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The Astronomical Register.

No. 169. JANUARY. 1877.

ROYAL ASTRONOMICAL SOCIETY.

Session 1876—77.

Second Meeting after the Long Vacation, December 8th, 1876.

William Huggins, Esq., D.C.L., LL.D., &c., *President*, in the Chair.

Secretaries—Mr. Dunkin and Mr. Ranyard.

The minutes of the last Meeting were read and confirmed.

It was announced that 52 presents had been received since the last meeting, and the thanks of the Society were voted to the respective donors.

Capt. James Waterhouse, Assistant Surveyor-General of India,

was duly elected a Fellow of the Society.

Mr. Dunkin read a paper by Col. Tennant *On the error in the tabular place of Venus on December 8th,* 1874. Mr. Dunkin said that it would be remembered that in a late number in the *Monthly Notices* there was a short paper by Col. Tennant giving the errors of right ascension and north polar distance of the tabular place of Venus on the date of the late transit. It appeared that when Col. Tennant received the computations which the Astronomer-Royal has published, on comparing the Greenwich places with his own he found a difference of rather more than two seconds between the two, he had, therefore, made another computation, but can find no mistake in his former work, and yet is fully convinced that the Greenwich places are right. In writing to Mr. Dunkin he had mentioned the fact, and he thought it right to mention it in the *Monthly Notices.*

Mr. Dunkin said he had another paper which might be called a *reclamation* by Otto Struve. In a paper by Dr. Robinson reference had been made to the space-penetrating power of reflectors and refractors, and particular reference was made to a comparison between the relative powers of the 15-in. refractor at Pulkowa and Mr. Lassell's 4-ft. reflector, which M. Struve had had an opportunity of examining when it was in Malta. Dr. Robinson had spoken of the 4-ft. reflector as being decidedly superior in space-penetrating power to the Pulkowa refractor. But M. Otto Struve thought that the contrary was the case.

Mr. Lassell said: On the occasion of a visit of the late Mr. George P. Bond, of Harvard University, to this country, I conferred with him respecting the comparative powers of his 15-in. refractor (the same size as the Pulkowa telescope) and my 2-ft. reflector, and he voluntarily expressed his conviction that mine had the advantage. On my stating that I had been accustomed to consider the 2-ft. reflector as about equal to a 17-inch refractor he expressed his assent that it might be so. It will thus be inferred that my opinion totally differs from that of M. Otto Struve.

The President: I think I can confirm what Mr. Lassell has said, for on several occasions I have examined the satellites of Neptune and one or two nebulæ, first with the 2-ft. reflector at Mr. Lassell's observatory and subsequently at my own observatory with my 15-inch achromatic, which would have a greater light-giving power than the one at Pulkowa, for the surfaces of the two lenses are united by castor oil, and the glass of both lenses is of exceeding purity and colour, there being scarcely any shade or tint of colour, and the comparison would, therefore, I think be rather in favour of my achromatic. The result of my observation is that Mr. Lassell's 2-ft. reflector is in a small degree superior to the 15-in. and consequently the 2-ft. reflector being superior in space-penetrating power to the 15-in., it seems scarcely possible to believe that the 4-ft. reflector is not also much superior to the 15-in.

Father Perry was called upon to read two papers, one *On some diffraction experiments of M. Ch. André with reference to astronomical instruments and the general theory of diffraction*, and the other *On the phenomena exhibited by a planet in its transit across the solar disc, from observations made by M. Ch. André.* Father Perry said that M. André had started with the well-known fact that the image of a brilliant point, as seen in a telescope, consists of a central disc surrounded by a succession of rings which diminish rapidly in intensity. If the intensity of any point of the image is represented by an ordinate, and the distance of the same point from the axis of the telescope by an abscissa, a curve for the intensity may be formed and the rotation of this curve round the axis of the telescope will form what M. André calls the solid of diffraction, and he has shown that the brightness of the image of a uniform bright surface, as seen in a telescope, may be represented by the ordinates of a curved surface, the breadth of the base of which varies inversely as the diameter of the object glass or mirror. By means of this surface he had calculated the theoretical diffraction at various parts of the field of view, when a uniform bright surface was observed with a telescope of given aperture, and he had been making experiments to determine whether the black drop seen with a model of the transit of Venus corresponded with the magnitude of the theoretical diffraction as calculated by his method.

M. André was one of the astronomers sent out with the French expedition to observe the transit of Venus, and he has recently been making experiments at the Ecole Normale in preparation for the transit of 1882. M. André's experiments are being made under the most advantageous circumstances, the splendid subterranean basement of the Laboratoire being placed at his disposal. His model of the planet moves by clock-work, and the bright field which represents the sun is illuminated by a Drummond light. The conclusions he has arrived at are the following: a ligament or black drop is a necessary phenomenon under certain circumstances, and not merely accidental. With a source of light of sufficient brilliancy, the angular measures of the ligament are inversely proportionate to the object-glass. 2. It is always possible to get rid of the ligament, and reduce the phenomenon to geometrical contacts either by reducing the intensity of the source of light or augmenting the absorbing power of the dark glass. 3. Definition or imperfect focussing has but small influence on the results, the phenomenon changing little so long as a fair focus is obtained. 4. Atmospheric undulations have a considerable influence tending to give the image of the planet every description of strange form, but this difficulty may be got rid of by coating one of the surfaces of the object-glass with a thin film of silver. 5. Even when the object-glass is so small and the dark glass so thin that the ligament is still present, observations may be made very precisely. One phase of the phenomena is simultaneous for all apertures, and can be estimated to within 0·75 of a second of time for interior contact at ingress and $1\frac{1}{2}$ sec. for egress, thus giving a probably total error of $2\frac{1}{4}$ sec. on the observed duration of the transit. 6. In order to secure the solar parallax to within $\frac{1}{100}$th of a sec. it is sufficient that the error in the duration of time should not exceed five secs. of time. A small telescope may therefore serve to determine this parallax to within 0·005 of a second, and Halley was, therefore, not mistaken about the value of his method. 7. The above results agree with the theory of instrumental diffraction and can be rigorously deduced from it.

Father Perry said that the difficulty that had so far surrounded the observations of Venus arose from a preconceived idea that the phenomenon alone to be observed was geometrical contact which was only visible in a restricted number of cases. It would be well to determine accurately the exact thickness of the dark glass most suitable for the observations, and this might be attained experimentally at the previous transit of Mercury. Father Perry added that it would be well also to make experiments as to the tint of coloured glasses best suited for diminishing the light of the photosphere so that the light of the chromosphere might be rendered more visible, in order to give greater value to the first external contact, as M. Janssen had done. No doubt, as seen under ordinary circumstances, the first external contact was over long before anything could be seen of the limb of the planet.

Mr. Christie said that he thought that there was nothing very new in M. André's theoretical investigations as to diffraction. There was a paper by Dr. Bakhuyzen in the *Astronomische Nachrichten* for May, 1874, in which the whole phenomenon of the black drop was examined on the ordinary theory of diffraction. There are, however, many points of interest in M. André's investigations. His experiments as to the amount of irradiation with various apertures and degrees of illumination are of great importance. Measures of the irradiation in the case of the sun and moon and of the diameter of the diffraction discs of stars have been made long ago. The law for finding the diameter of the discs with various apertures is given in the Astronomer-Royal's tract on the theory of light, and actual measures of the diameter of the discs of stars of different magnitudes and colours have been made more recently by Mr. Knott, and these seem to correspond pretty well with the calculated diameters of the discs. But I do not think that by experiments we shall ever deduce the probable error of the actual observations of the transit, because what is seen in the actual transit is a very different thing from what is seen in the model. All the principal nations that have sent out expeditions have used models somewhat like M. André's, but when the observers saw an actual transit they were fairly non-plussed. The real difficulty they experienced, so far as I can gather, arose from the bright atmosphere of Venus, which appeared as a ring of light around the planet, and so made them think that contact was taking place before it really did.

The same thing happened in the last transit in 1769. Although I have felt it necessary to point out that others have worked on this question, I must say that M. André's results seem to be important as a continuation of former researches, because the question has not been thoroughly worked out in its details, though its principles are well understood. I do not think there is any difficulty in explaining the phenomena theoretically, but it is a practical question to find to what extent the irradiation varies with the brightness of the object.

Mr. Ranyard: I have not read M. André's paper, but as far as I understand it he has dealt chiefly with what may be called the physical irradiation, and not with the irradiation due to physiological causes, or due to the imperfections of the eye as an optical instrument. The irradiation arising from this latter cause is in the case of bright lights far greater in amount than the physical irradiation or diffraction fringe. M. André's observations were made with a model illuminated by a lime-light, but in order to determine the angular magnitudes of the black drop, as seen at a transit, it would be necessary to make experiments with the sun as a background, and even then great allowance would have to be made for the varying brightness of the sun as seen at different altitudes and in different states of the weather, as well as for the effect produced by the atmosphere at the limb of the planet, which possibly may have a disturbing effect at internal as well as external contact. As to Father Perry's remark about the body of Venus seen projected on the corona, before external contact, it seems certain, when we consider the brightness of the corona, that M. Janssen must have been deceived by seeing what everybody else observed as the illuminated line of light round the body of Venus.

Capt. Noble: There is one point in M. André's *resumé* which possessed to me a considerable amount of interest. He says that the correction of the moon's diameter to be deduced from theory corresponds with the correction of the diameter found experimentally from occultations, and which has long been used at Greenwich. An attempt has recently been made in what I suppose I may now call our standard English book on the Moon to prove the existence of a lunar atmosphere from the discrepancy between the diameter derived from occultations and the diameter of the bright moon. It would seem from M. André's paper that the corrections to be applied may be got at within a few hundredths of a second; which, assuming his data to be correct, quite disposes of any argument for a lunar atmosphere.

Mr. Marth: I may remind you that Bessel in observing the transit of Mercury in 1832, was very intent on discovering anything like an effect of irradiation, and was not able to discover any sensible amount with his 6-inch instrument, though it was very sensible in telescopes of only 3 or 4 inches in diameter. Capt. Noble has referred to the moon's atmosphere. We must not forget that in order to deduce this very considerable atmosphere of the moon, assumed in the book he has referred to, it would be necessary to bore a hole into the rim of the moon five miles in depth. It cannot be assumed that the rim of the moon is something like five miles higher than the general surface of the moon.

Father Perry: One word as to what Mr. Ranyard has said. I think I could not have made myself understood. In referring to the use of certain tints of glass, I did not mean that it was for the purpose of seeing the chromosphere, but I referred to the observations, which are not sufficiently known, of M. Janssen, who observed the external contact perfectly well at Nagasaki. He used certain tints of glass and saw Venus clearly, a good number of seconds before the contact, and gave notice of it to his assistant. He has not given his friends a notion of the sort of glass he used, nor can anything be got out of him as to the colour. But why should we not study the different tints of glass in order to be ready for observing the external contact ourselves?

Mr. Ranyard: I think that M. Janssen in one of his early telegrams stated that he used blue glass, and that his reason for doing so was that the spectrum of the corona consisted largely of blue rays, but it is hard to understand what he meant by this, for the spectrum of the corona is a continuous spectrum with one faint green line—we may leave out of consideration the faint hydrogen lines—and there is no reason that I know of to suppose that the continuous spectrum is richer at the violet end than the ordinary solar spectrum. We know that the whole light of the corona is very faint compared with the light derived from the atmosphere between us and the sun, so faint indeed that during a partial eclipse the limb of the moon cannot be detected upon the corona till a few minutes before totality, when the solar crescent is very thin, and the illumination of the atmosphere must be enormously decreased. It seems very improbable, therefore, that by dimming the light of the atmosphere as well as that of the corona with a blue glass, the body of Venus should be rendered visible.

Mr. Marth: If I remember right, in 1815 the Assistant-Secretary of the Royal Society, Mr. Lee, suggested that a series of measures should be made of the sun's diameter as seen with glasses of various colours, but it turned out that the difference of diameter was not sufficient to be detected.

Mr. Ranyard: Secchi has also, I believe, measured the solar diameter with red and blue glasses.

Mr. Marth: Perhaps Secchi may have been more successful than Mr. Lee, but when these experiments by Lee were repeated, it turned out that as far as ordinary observations go the differences were never sensible. I wish to refer to these experiments, as I think it only due to Mr. Lee to mention that he was alive to the question.

The President: I will ask you to return our best thanks to Father Perry. Although the theory may be completely understood, it is quite to be expected that the observations now made in France may lead to results of importance with regard to the observations of the coming transit.

The President: I may perhaps be permitted to occupy the attention of the meeting for a few minutes. It may possibly be known to the Fellows that I have been endeavouring for some time past to apply photography to the spectra of the stars. As far back as 1862 I obtained a photograph of the spectrum of Sirius, but for certain reasons the lines of the spectrum could not be distinguished, it was merely a strong dark streak upon the plate. In the last year, after the trial of a variety of methods, I have fairly succeeded, and I thought it would interest the Fellows of the Society to see an enlarged copy of a photograph of the spectrum of Vega which I have taken. The original spectrum is about half an inch in length, and so well defined that it can be examined and measured under a microscope. There is also the solar spectrum taken on the same scale. Though it is only half an inch long, you can see many lines between the two H lines showing the extreme precision of definition of which the apparatus is capable. After trying a variety of processes the advantage has been found to be in favour of dry plates for one reason, among others, that after taking the spectrum of a star at night on a dry plate, the dry plate can be allowed to remain in the instrument, and then the next morning the part of the slit which was closed at the time of taking the spectrum of the star can be opened, and the other part used for the star now being closed, the solar spectrum can be taken upon the plate above that of the spectrum of the star so as to permit of accurate comparison between the lines of the solar spectrum and the lines of the spectrum of the star. In a similar manner it is easy to photograph a comparison spectrum obtained with the electric spark so as to give direct comparison with the lines of a metal. In this drawing the upper part represents some of the lines of the solar spectrum. In the photograph the solar spectrum is crowded with a number of lines, and below you have the spectrum of Vega, which begins about G. of the solar spectrum, and the lines of this particular star go about as far as the line M. of the spectrum in the portion which is visible by fluoresence. It is well known that the spectrum of Vega shows strongly the lines of hydrogen, F. and C., and also a third line in the blue that can be seen by ordinary eye observers. This photographic spectrum takes up the spectrum where the eye observers leave off, and here you have the third line of hydrogen beginning with the line near G., a fourth line of hydrogen and five other strong lines. I have also spectra of one or two other stars and also of the planet Venus, and also of the moon, which by very careful comparison with the solar spectrum would afford an accurate method of ascertaining whether any modification is produced upon the sun's light by reflection at the lunar surface: I am not at the present moment prepared to speak upon this point. (Hear, hear.)

Mr. Dunkin: We will now go back again to the transit of Venus. I have a paper by Mr. Stone, of the Cape of Good Hope. It is a very long paper, and it will only be necessary to read a few extracts with regard to the black drop. Mr. Stone says that when the observations of the transit of 1874 are published he hopes to examine them in detail with reference to the internal contact bearing on the determination of the sun's distance; but as there is an inference which has been hardening into a conviction that none of the skilled observers of 1874, who observed the internal contact, saw what is called the black drop, it is desirable that it should be pointed out that such is not the case.

Mr. Wilson has also sent us a paper containing a list of binary stars and other interesting double stars. It is desirable that some more information should be given in such star catalogues than is given in the present paper. What is wanted is a list with complete information with regard to each star in it.

Mr. Christie was called upon to read a paper on *The gradation of light upon the disc of Venus.* He had been making some further measures with his polarizing eye-piece, and gave upon the black board the measures which he had obtained of the brightness of different parts of the disc. The brightest portion was a nearly stellar point seven times as bright as the edge of the disc; around this was a circular area and outside that an oblong area of decreasing brightness, which he severally estimated as 1·5 and 4·8 times brighter than the edge of the disc near to the terminator. He thought that there could be no doubt that there was a considerable gradation of light from the central or nearly central spot, and it seemed difficult to explain this appearance unless by supposing that there was specular reflection modified by atmospheric diffusion.

Mr. Ranyard: Do the elliptic areas you have been describing correspond with the sausage-shaped area you spoke of last time?

Mr. Christie: I do not think that I used the term sausage-shaped area. The oblong patch is the one which corresponds to it. As the planet becomes more gibbous, then the concavity on the inside of the bright area becomes nearly straight. My first observation of Venus was in October, but a month later the planet would be more gibbous, and there was then a more sensibly oblong patch with round corners about three times as long as broad; and on reducing the light a third at each end disappear, and it became perfectly circular. I was not prepossessed with that view because I expected it to get narrower and narrower, but it did not do so. It got perfectly circular, and then ultimately came to a point; afterwards I considered the matter and found that that is what one would expect.

Mr. Ranyard: Would not you expect to find the boundary of such an area of equal illumination, as seen in projection, to be an ellipse?

Mr. Christie: I am not disputing it, but practically Venus was so nearly full that the deviations from the true circular form would be insensible.

Mr. Ranyard: I do not see how the double-curved area can be accounted for on the theory of specular reflection only. I could understand the circular or elliptical, but not the sausage-shaped area.

Mr. Christie: I think if Venus was really crescent-shaped, and not past quadrature you might get that form, but the recent observations were made when she was gibbous.

Mr. Ranyard: When Venus is crescent-shaped with specular reflection you would expect to find the maximum illumination apparently very close to the limb, and if the light were dispersed by a rough surface you would expect to find the maximum of illumination actually on the limb, that is, on the part of the planet where the sun is in the zenith.

Mr. Neison: What Mr. Ranyard has been saying is, I think, similar to what is contained in a note of mine before the Society, which shows the effect of brightness which is to be expected if the surface of Venus were rough like that of the moon. The point where the sun is in the zenith, as seen from Venus, would then be the brightest, and the areas of equal illumination would be elliptic areas round this point, so that Mr. Christie's observations might be accounted for without having recourse to the theory of specular reflection.

Mr. Mattieu Williams read a paper on Professor Langley's paper read at the last meeting as to the effect of the radiation of solar spots on terrestrial climates.

Mr. Marth drew attention to the importance of making observations at the present time of the position of the companion of *a* Centauri. He showed a diagram giving the positions of the companion star as observed during the last 50 years. From this it appeared that the small star had been last year at or near its periastre. and was consequently moving with great rapidity especially in position angle. Though the importance of obtaining observations for some years past was known to all who were acquainted with Mr. Powell's elements or with Mr. Hind's ephemeris in the *Monthly Notices* for November, 1872, it seemed that the sonthern observers had neglected their splendid opportunities, and he applied, therefore, to any of their personal friends who might be present to give their help by exercising a little friendly pressure. The best time might indeed have gone by, but a time of grace was still given, and he hoped that observers in the southern hemisphere would not fail to take advantage of the opportunity.

Mr. Dunkin suggested that Mr. Marth should write a note on the subject, to be inserted in the *Monthly Notices,* this would bring the matter under the observation of the observers at the Cape, Melbourne, and elsewhere.

Mr. Marth said that *a* Centauri may possibly have been observed at Melbourne, but it is not very likely, and if the present opportunity is neglected there would be no other until the middle of the next century, and then this generation may be blamed for not having made observations.

The President said he hoped Mr. Marth would write a few words for the *Monthly Notices.* It would no doubt be the best course in order to draw attention to the subject.

The following paper was also announced and partly read:—

On an oversight in La Place's Mécanique Céleste, and on the interior density of the Planets: by George Darwin.

The Meeting adjourned at ten o'clock.

At the last meeting we omitted to state that Mr. Thornthwaite exhibited a new form of altazimuth stand.

M. YVON VILLARCEAU ON THE REDUCTION OF THE PHOTOGRAPHS OF THE TRANSIT OF VENUS.

Amongst the "Additions" to the *Connaissance des Temps* for 1877, appears a paper by M. Villarceau entitled "Recherches sur l'emploi des Photographies recueillies dans les observations du Passage de Vénus." In this memoir the author applies the theory of probabilities to the problem, as being the method calculated to give the most exact results.

M. Villarceau first supposes the micrometer measures of the co-ordinates of the outlines of Venus and of the Sun to be corrected for refraction (he gives his method of applying this correction at the end of the memoir), then supposing $F(x, y) = 0$ to be the equation to a plane curve of which the form is known, and the parameters of which are a, b, c, &c., the theory of probabilities leads us to the condition that $\Sigma(\Delta x^2 + \Delta y^2)$ is a minimum, Δx and Δy being the differences between the observed and calculated values of x and y. But $\Delta x^2 + \Delta y^2$ is the square of the distance between the calculated and observed place, and if this distance be projected on the normal drawn through the observed point and on the tangent at that point, and ν and τ be the two projections, then $\Delta x^2 + \Delta y^2 = \nu^2 + \tau^2$ and the equation of condition becomes $d.\,\Sigma\,\nu^2 + d.\,\Sigma\,\tau^2 = 0$.

The author then proves, at considerable length, that if x and y are only connected by the relation $F(x, y) = 0$, we obtain the most probable solution by making use of the condition $d.\,\Sigma\,\nu^2 = 0$ which he shows to be equivalent to

$$\Sigma\,\frac{F \cdot \dfrac{dF}{da}}{\dfrac{dF^2}{dx^2} + \dfrac{dF^2}{dy^2}} = 0$$

with similar conditions to be obtained by substituting for a the other constants b, c, &c.

The next step is to apply this result to the circle $(x - p)^2 + (y - q)^2 - \nu^2 = 0$, first transforming the co-ordinates by putting $x = x_m + \xi$, $y = y_m + \eta$: x_m and y_m being the means of abscissæ and ordinates of m observed points. By this means, using the conditions given above, we get the co-ordinates of the centre and the radius in terms of known quantities. If then the above circle represents the outline of Venus, M. Villarceau showing that the error is inconsiderable if we suppose the Sun and Venus to be circular, and let $(x - p')^2 + (y - q')^2 - \nu'^2 = 0$ be the equation to the circular outline of the Sun, by a similar process to the above we obtain the co-ordinates of the centre p', q' and the radius ν'; and thus finally the distance of centres of Sun and Venus which leads to the solution of the problem. The practical steps to be gone through are the following :—

The value of a revolution of the micrometer screw of the measuring apparatus is to be corrected for refraction for each photograph separately. Then, considering the photographic image of each body by itself, m points of the outline of one of them are to be observed, avoiding points in the near neighbourhood of intersections or of contact of the two discs, as according to M. Villarceau, no reliance can be placed on them. If x_m, y_m be the means of these then $x_m = \frac{1}{m}\,\Sigma\,x$, $y_m = \frac{1}{m}\,\Sigma\,y$, and we substitute for x and y the co-ordinates ξ and η making $\xi = x - x_m$ $\eta = y - y_m$, so that $\Sigma\,\xi = 0$ and $\Sigma\,\eta = 0$, and proceed to find the co-ordinates of the centre and the radius from the formulæ given in the Memoir. The same process must be gone through for the other outline, and then, from these results, the distance of the centres has to be found. As it is necessary for all these computations to be made for each photograph we fear the practical labour of reduction, by this method, would be very great; and however elegant it may be considered from a theoretical point of view, we imagine that few people would be willing to make use of it in practice.

EXPLANATION OF NAMES ON MAP OF THE MOON.

(Continued from Vol. XIV. page 260.)

Lahire (M.) French math., d. 1719.
Lambert (S.) Germ. philos. and math., d. 1777.
Lansberg. Dutch math., d. 1632.
Langrenus. Astron., d. 1644.
Legentil. Fr Astron., d. 1792.
Lehmann (M.) Germ. astron., 19th century.
Le Monnier (L.) Fr. astron., d. 1799.
Lexell (M.) Germ. astron., d. 1783.
Licetus. Ital. physicn. and philos., d. 1656.
Lichtenberg (M.) Germ. astron., 18th century.
Lilius. Q. Lily the astrologer, d. 1681. (?)
Linné (M.) Linnæus, botanist, d. 1778.
Littrow (M.) Germ. astron., d. 1840.
Longomontanus. Danish astron., d. 1647.
Louville (S.) Fr. astron., d. 1732.
Lubiniezky (S.) Q. Polish theologn. (Theatrum Cometicum), d. 1675 (?)

Macrobius. (Saturnalia), middle of 5th century A.D.
Magelhaens (M.) Portuguese navigator, d. 1521.
Maginus. (Magini) Math. and astron., d., 1617.
Mairan (S.) Fr. nat. philos., d. 1771.
Malapert. Math. and poet, d. 1630.
Manilius. Latin poet (astronomicon), time of Augustus Cæsar.
Maraldi (S.) I. Math. and astron., d. 1729. II. d. 1788. III. d. 1797.
Marco Polo (M.) Venetian traveller, d. 1323.
Marinus (M.) Gr. geogr., A.D. 150.
Marius. Astron., d. 1624.
Mason (M.) Astron., d. 1787.
Maupertuis (S.) Fr. math. and astron., d. 1759.
Maurolycus. (Maurolico) Of Messina, math. d. 1575.
Medii Sinus (M.) Bay of the Middle.
Menelaus (S.) Alexandrian geometer, A.D. 98.
Mercurius. Q. Hermes Trismegistus, reputed author of a variety of works (?)
Mersenius. (Mersenne) Fr. math., d. 1648.
Metius. Adrian M., math., d. 1635. J. Metius, his brother, reputed inventor of the telescope, 1609.
Meton. Athenian astron. (Lunar cycle), B.C. 432.
Mortis Lacus. Lake of death.
Mutus. Astron., d., 1673.
Explanations wanted: Manzinus (R.), Messala (R.), Milichius (R.), Moretus (R.), Mösting (M.).

***Nasireddin* (M.) Persian astron., d. 1276.**

Neander Germ. physn. and math., d. 1581.
Nearchus (M.) Gr. admiral, accompanied Alexander the Great.
Nebularum Palus. Marsh of vapours.
Nectaris (Mare). Sea of Nectar.
Neper (S.) Or Napier, inventor of logs, d. 1617.
Nicolai (M.) Germ. astron., 19th century.
Nicollet (N.) Fr. astron., 19th century.
Nonius. (Nunez) Portuguese math. and astron. d. 1577.
Nubium Mare. Sea of clouds.

Œnopides. Of Chios, math. and astron., contemp. with Anaxagoras (?).
Oersted (M.) Danish electro-magnetist, d. 1851.
Oken (M.) Swiss naturalist, d. 1851 (?)
Oriani (M.) Ital. astron., d. 1832.
Orontius. Orontius Fineus, astron., d. 1555.

Palitzsch (S.) Astron., near Dresden, d 1788.
Pallas (M.) Germ. naturalist and traveller. d. 1811.
Parrot (M.) Russian traveller; ascended Mt. Ararat in 1829.
Petavius (Petau). Chronologist and Theolog., d. 1652.
Philolaus. Pythagorean philos., 5th century, B.C.
Picard (S.) Fr. astron. and math., d. 1682.
Piccolomini. Ital. astron., d. 1553.
Pico (S.) of Mirandola. Universal scholar, d. 1494.
Pictet (M.) Genevese nat. philos., d. 1825.
Pingré (S.) Fr. astron., d. 1796.
Pitiscus. Theolog. and math., d. 1613.
Polybius (M.) Gr. historian, d. cir. B.C. 122.
Pons (M.) Fr. astron., d. 1831.
Posidonius. Gr. philos. and astron., d. B.C. 50?
Procellarun Oceanus. Ocean of storms.
Proclus. Neo-platonic philos., d. A.D. 485 at Athens.
Prom. Ænarium. Q. in the island of Ischia, anciently Ænaria ?
Ptolemæus. Alexandrian math., astron. and geogr., A.D. 150.
Purbach. Germ. math. and astron , d., 1461.
Putredinis Palus. Marsh of Corruption.
Pythagoras. Gr. philos., B.C. 540-510.
Pytheas, of Marseilles. Gr. navigator and discoverer. Time of Alexander the Great.
Explanation wanted: Pentland (M.), Pitatus (R.), Piton (B.), Pontanus (R.)

(*To be continued.*)

REVIEW.

Handbook of Astronomy. By Dionysius Lardner, D.C.L., formerly Professor of Natural Philosophy and Astronomy in University College, London. Fourth edition, revised and edited by Edwin Dunkin, F.R.A.S., of the Royal Observatory, Greenwich, and Honorary Secretary of the Royal Astronomical Society. With 38 plates and upwards of 100 illustrations on wood. London: Lockwood & Co., 1876. 9s. 6d.

Having used Lardner's work, as it first appeared, for many years, we viewed with pleasure this compact and nicely got up volume, not doubting that the editorial care of such an astronomer as Mr. Dunkin would have made many improvements. Before, however, turning over both editions together, page by page, to judge of the additions and corrections, we feared from the smaller size of this new one that we should miss a considerable part of the original work. Accordingly, comparing the number of pages, we found that the latter seemed to have been reduced by one-third. In fact, however, a slightly smaller type and closer printing considerably qualify this estimate. Still, there is much of Lardner's work that does not appear in this edition. The following six chapters—xix. to xxiv. have been left out. They comprise, *Theory of Variable Orbits*, *Problem of the Three Bodies*, *Lunar Theory*, *Theory of the Jovian System*, *Theory of Planetary Perturbations*, *Theory of Spheroidal Perturbations*. In the preface we are told that, "In the body of the work, chiefly on account of the limited extent of the volume, subjects which require some amount of mathematical elucidation are necessarily omitted. Those of our readers who wish to enter upon the study of the theoretical branches of the science, such as the theory of planetary perturbations, or the lunar theory, &c. should select those works which are specially prepared for students in the universities, on each of these important subjects. Our limits will not allow us to include them without sacrificing other instructive matter which agrees more with the objects and intentions of this work. The reader will, however, find inserted in the concluding chapter, further explanations on a few questions which in an earlier portion of the work have been partially treated on in the text." We are doubtful from this whether the limits of the work were fixed by the publishers or the editor, or both together, but anyhow we regret the abridgment. There are already good elementary books suited to those who might be scared by the appearance of formulæ, and there must be more persons now than in Dr. Lardner's time capable of appreciating the amount of mathematical illustration that was embodied in his work. There is little doubt that many a student values an insight into the more abstruse departments of the science, an elementary introduction to which would often allure him to follow them up in advanced treatises. Besides many formulæ either omitted altogether or relegated to the concluding chapter the author's abridged account of the Uranography of Saturn is left out. The reader is referred in a note to the extended paper on this subject by Dr. Lardner in the Memoirs of the Royal Astronomical Society, but these memoirs are inaccessible to most persons, and we think it a pity that a curious subject has been dismissed as "speculative." And as to controverted matters, an editor need not feel himself responsible for the treatment of *every* subject in another man's work; nor is he obliged in every case to give an express opinion of his own. We conclude these remarks on the omissions, only saying that we would willingly have paid half as much again for the volume, if it had contained most of what has been left out.

A different and improved arrangement of the chapters and subjects from the original work has been made, and almost every page testifies to the pains which have been taken to correct the numerical quantities, and to give the tabular numbers representing the elements of the solar system, &c., according to the best and most recent authorities. Probably there is no other work in our language (nor, perhaps, in any other) which in this respect embodies such copious and accurate information; and for this, the result of much teasing and wearisome calculation, the best thanks of astronomers are due to Mr. Dunkin. This volume will save much trouble in searching for particular elements in many books, and with a frequently unsatisfactory result. It will, indeed, we think, become almost indispensable. It is needless to observe that where mistakes or misprints occur in Lardner's work they have been rectified. Some of these are glaring enough, *e g.*, the numbers relating to the moon's hourly motion, p. 187 of the original; and the scale of the orbital motion and distance of Saturn, p. 360. By the way, we notice a misprint in the present edition, p. 279, line 26, where "intermediate" should of course be "indeterminate." The value of the sun's parallax used in the calculations in this volume is 8·″94, which cannot be far from the truth; and the editor rightly judges that it is best to adhere to this value till the observations of the transits of Venus of 1874 and 1888 definitely settle the point.

As far as we can see, the work is pretty well brought down in the way of additions to the actual state of astronomy. The editor might, perhaps, sometimes have gone a little further than he has: for instance—recent speculations on the physical state of Jupiter and Saturn might have been mentioned; and we might have had a little more about the stellar universe, and something about star-drift. The account of the minor planets, or as they are called in the book "planetoids," is very full, more so than in any other book we know of. In chapter xv. 146 are treated of, and the after discovery of two more is mentioned in a note, p. 9.* Most of the plates and diagrams, &c. of the original are reproduced in this new edition, and some new ones are given, as the illustration of the Cavendish experiment, of Foucault's pendulum experiment, and the gyroscope. The execution of some of the plates is rather coarse, and not equal to those in the old work. Compare for instance plates xv. and xvi., the solar spots observed by Pastorff, with the corresponding vi. and vii. in the original, and the view of Saturn by Dawes, xxi., with xii. in the original, the superior delicacy of which is obvious. It seems difficult to prevent engravings of this sort being often too pronounced in their features; and sometimes they exaggerate an approach to symmetry in form and details where it exists in the drawings.

In concluding this notice of an unquestionably valuable work, we would suggest to writers and editors of astronomical books the great usefulness of referring their readers to other works in which they may find a more exhaustive treatment of the various subjects. This is not done nearly as often as it might be. At the conclusion of most of the chapters in the work in question it ought not to be difficult to mention various books, not only English, but American and Continental, of which the reader would often be glad to know. One who has access to such a collection of books as is found in the Royal Observatory could easily do this. And when some branches are omitted altogether, as the planetary perturbations and the lunar theory in this instance, a reference, for example, to such works as Cheyne's Planetary Theory, The Lunar Theory by Godfray, Sir George Airy's Treatise on Gravitation, and Mr. Proctor's recent work on the moon, &c., would have been desirable.

Dionysius Lardner, *redivivus*, would no doubt be gratified at his book having got into a fourth edition. He could not but rejoice at the removal of its blemishes, and appreciate the improvements which will make it a most welcome addition to the library of the amateur and even the professional astronomer. Whether he would exactly approve of the lopping to which it has been subjected may be questioned. But the tree is a flourishing one; and though to our thinking it has been deprived of some fine branches, the grafting has been very beneficial:

"Miratuoque novas frondes et non sua poma,"

Or, if the fruit is not new, there is more of it, and it is of better quality.

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* The discovery of (162) and (163) was mentioned in the June number of the *Astronomical Register*.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

SPECULAR REFLECTION FROM VENUS.

Sir,—Owing to the lateness of the hour at which Mr. Christie's paper on the alleged specular reflection from Venus was read, at the last meeting of the Royal Astronomical Society, I thought it expedient not to prolong the sitting by any oral remarks. This, however, was my only reason for remaining silent; and lest it should be imagined that, in my case, "silence gives consent" to the inference that Mr. Christie's observation in any way supported the supposed one of Mr. Brett, I would crave a very little space in your columns to show that it most certainly did *not*.

Passing over the not unimportant fact that Mr. Christie's conclusions themselves are questioned by such competent authorities as Messrs. Ranyard and Neison, I would just point out that his observation of a spot of light was made at a time when Venus was sufficiently gibbous to have brought that spot (supposing it to have had its origin in specular reflection) upon the disc of the planet—although not, it would seem, where he placed it: but in Mr. Brett's case his assumed observation was made when the planet was in quadrature; when of course *specular reflection could only take place from the limb itself, and must, ex necessitate, be invisible to a spectator upon the earth!* The most rudimentary acquaintance with optics will enable any one of your readers to construct a diagram of the sun, the earth, and Venus, when the latter is in quadrature; and to satisfy himself by mere inspection of the impossibility of specular reflection from Venus being perceptible at that time to a terrestrial observer. Mr. Christie may—or may not—have observed specular reflection: it is physically impossible that Mr. Brett can have done so.

I have the honour to be, sir, your most obedient servant,

Forest Lodge, Maresfield, Uckfield: WILLIAM NOBLE.
December 15, 1876.

METEOR.

Sir,—Among the "Astronomical Occurrences" for this month in the current number of the *Astronomical Register*, a close approach of the moon to the star ε Arietis at 9h. 33m. p.m. this evening is announced. I was unaware of this conjunction, and had no intention to observe it, but had seized the opportunity of a bright starlight sky for a brief watch for any Andromedes, or meteors of Biela's comet which might be making their appearance, when at 9h. 53m. a very fine meteor began its course at the lower edge of the moon, and passed through an arc of about 35° to near the star γ Pegasi. Aligning the positions carefully by surrounding stars and referring them to a star map, the point of first appearance was at R.A. 43°, N. Decl. 20°, and the point of disappearance at R.A. 7°, N. Decl. 14°. The first of these positions borders so closely upon the star ε Arietis that I was not surprised to find, on consulting it, that the moon's close approach to that star to night is mentioned in your list of

"Astronomical Occurrences for November." As it may have engaged the attention of telescopic observers elsewhere at about the hour of conjunction, it is not impossible that the meteor also may have been noticed and its apparent path recorded from some other points of view, which rather than its great brightness leads me to send you a description of its course. It was not brighter at first than a first magnitude star, of orange colour, and moving so slowly as only to reach a distance of five or six degrees from the moon in about one second, when instead of disappearing quickly it continued its course with little change of appearance for 10° further, drawing some orange sparks behind it and becoming white at last as it began to increase in brightness. In the last half of its course its brightness at about mid-distance surpassed that of the planet Jupiter, but scarcely equalled that of Venus, and its colour and appearance was that of a white Bengal-light or port-fire darting along and dying out by degrees at last. The speed in this part of its course was too great to distinguish a figure, or if it had a tail of sparks; and if it left a persistent streak on its track it was too faint to be visible in the strong moonlight. The duration of its flight was 3½ seconds, and its increasing speed at last suggests a foreshortened path approaching from a great distance where it was first visible, and where it appeared to be moving very slowly. We may accept this conclusion as probable, as we have no reason to suspect that the real speed of its motion can have undergone any very perceptible variation during the time of its appearance.

On this supposition its radiant-point, or the apparent position in space of the earlier remote parts of its path from which it came, was somewhere along its apparent path prolonged backwards not very far behind the point where it first appeared; and as when carried back 20° it passes about midway between Aldebaran and Pleiades, it is most probable that the meteor was a "Taurid," with a radiant-point not far from the latter place (at R.A., 60°, N. Decl. 21°), very near which the meteor shower proceeding from Taurus in November is known to have its centre of divergence. Several large meteors seen this month appear to have belonged to it, although the accounts are not very precise. The long horizontal flight from east to west of the large fireball seen in twilight on the evening of November 8th, corresponds with the position of this radiant point, then rising in the east; and a bolide seen in France on November 5th, disappearing with a flash brighter than moonlight near *α* Ursæ Majoris, which left a long luminous line on its course commencing near Capella, is directed from the same radiant point. Other similar records appear to indicate that for conspicuous-looking meteors, moving slowly with long courses it was an active shower during the first ten days of the present month, and it would be interesting to establish the true radiant point of the bright meteor seen this evening, by any corresponding observations of it which may have been made elsewhere, to ascertain if this notable system of November shooting stars is not confined entirely in its appearance, as it is generally supposed to be, to the first half of the month. The single observation of the meteor by itself is inconclusive, but it yet affords some grounds for the supposition that the shower may show signs of existence even longer than the enduring period, from October 25th—November 21st, assigned to it by Mr. Greg in his Catalogue of meteor radiant points, published by the British Association. In some maximum years of its display it may possibly contribute the bright durable-looking meteors of its stream, not only to the meteor spectacle of the Leonids on November 14th (when it first attracted notice), but also to that of the much later "Andromedes" of

November 24th — 30th, when I am not aware that any proofs of its existence, if they occur so late, have hitherto been recognised. I saw no other shooting stars in a short watch of half an hour.

I remain, sir, yours faithfully,
Newcastle-on-Tyne: A. S. HERSCHEL.
November 29, 1876.

NOVEMBER AND DECEMBER METEORS.

Sir,—The following is the result of my observations of the "Taurid" shower, during the past month.

The first meteor was seen on November 6th, and they continued until the first week in December.

In about 15 hours' watching between these two dates, I observed 54 meteors apparently belonging to this family.

The radiant point, deduced from several centres of radiation, was at R.A. 62°, Dec. + 22°, between the Hyades and Pleiades.

In general the meteors were small and rather slow, frequently of an orange colour, with short trains, none, however, leaving streaks.

On the 9th, 10th and 16th when most were observed, the horary number of Taurids was not more than 5 for one observer, or 13 including meteors from other radiants.

The "Geminid" shower appears to have been very good this year, but unfortunately the nights of the 9th and 10th were cloudy and the 11th almost entirely so; 40 meteors were, however, seen on the last date, a clear interval of 40 minutes giving 16 meteors from Gemini. The first meteor of this family was noted on December 4th and they have continued to the present time.

Ninety-six "Geminids" were seen in about 13 hours' watching, from the 4th to the 12th of this month, almost all radiating from a marked position between α and θ Geminorum, at R.A, 107°, Dec. + 35½°, a few, however, radiating from near Pollux.

In appearance they were generally quick, short, and white without trains or streaks; except in the case of the larger ones: 6 first magnitude meteors being lemon-yellow in colour, some leaving streaks and others having trains. Several 2nd magnitude ones were bluish in colour.

On the 11th and 12th, the horary number of "Geminids" only was about 13 for one observer, or 16 when unconformable meteors are included.

Though very clear on the 13th, but few meteors were seen, of which one or two were possibly from the radient in Gemini.

Writtle, Chelmsford: H. CORDER.
December 14, 1876.

NOVEMBER SHOOTING STARS.

As a *resumé* of my observations (*Register*, p. 296), I may mention that in November watching for 25¾ hours, I saw 212 shooting stars, and registered 183 of them. The radiant in Cassiopeia (No. 2 Oct. list) was continued at R.A. 17° Dec. + 53° (7 meteors), that in Lynx (No. 5) at 123° + 45° (6 meteors), that in Lacerta (No. 14) at 344° + 42° (4 meteors), that in Camelopardus (No. 9) at 69° + 66° (9 meteors), that in Taurus (No. 15) at 60° + 19° (19 meteors), and that in Gemini (No. 16) at 111° + 22° (5 meteors). The Taurids were a fine shower in the early part of the month and the radiant at H. 17, Camelopardus, was also very active. On the nights of the 12th—14th clouds prevented observation, but I saw 5 of the Leonids on the morning of the 20th and 21st, indicating the radiant accurately at 149° + 22°, and I can find no reference to their having been observed so late before. The Geminids, or noted December shower with max. about December 11, commenced on the early morning of the 21st. The chief shower in operation during the latter part of November was from Leo Minor at 155° + 36° (21 meteors). There were two other radiants near this position, viz., at 123° + 45°, and 137° + 63° (9 meteors), and they make up a fine group giving very similar meteors, rapid and white, often leaving persistent streaks marking their courses and reminding one of the Leonids. There was also an active radiant near α Aurigæ, 78° + 43° (9 meteors). Many other showers manifested themselves less strikingly. In the mornings between 20th—28th, I watched for long periods, and found four new showers as follows:—

near η Ursæ Majoris	208° + 43°	12 meteors.
near α Boötis	212° + 18°	7 meteors.
near τ Leonis	170° + 4°	6 meteors.
in Sextans	153° — 1°	6 meteors.

These radiants come into action just before sunrise, and this may account for their having escaped detection before. The one at η Ursæ was very active, supplying small short meteors, somewhat rapid and without trains. I also found a radiant (on Nov. 28 only) near Coma Berenices at 176° + 26° (4 meteors), and that also appears to be a new position.

I have lately seen shooting stars very much more numerous in the mornings than in the evenings. Watching for 13¾ hours *before* midnight I observed 79; while watching for 12 hours *after* midnight I observed 133. Thus I have seen double the number *a.m.* than *p m.* I found the same thing evident from my observations in October, though during the other months I have done little work in the mornings and cannot make the same comparison. Lately, however, I have found there is a progressive increase in frequency as the night advances, being at a minimum in the early evening hours, and at a maximum before the morning twilight.

As a class the meteors of the last two months have been small, and the great majority of them white and rapidly moving. The following are the magnitudes of 306 registered during October 13—Nov. 28.

× or = 1st mag.	2nd mag.	3rd mag.	4th mag.	5th mag.	6th mag.		Total.
19	49	61	107	65	5	=	306

WILLIAM F. DENNING.

Ashley Down, Bristol:
December 5, 1876.

P.S.—Dec. 15.—During the first half of this month I registered 117 meteors, and saw 133 altogether, in 17 hours' watching, chiefly p.m. The two finest showers were (1) from α Geminorum (the Geminids) R.A. 106° + 32° (37 meteors) and (2) from ζ-β Tauri (the Taurids II.) R.A. 80° + 23° (20 meteors). The former was well seen (and very active) on the 12th, the latter on the 6th. The meteors of both showers were, generally, slow and bright. That in Taurus seems an entirely new radiant. As a class the meteors of December have been much finer and slower than those of October—November.

W. F. D.

ASTRONOMICAL OCCURRENCES FOR JANUARY, 1877.

DATE.			Principal Occurrences.	Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Capella.
Mon	1	14 32 15 30 18 15	Occultation of 80 Cancri (6½) Reappearance of ditto Near approach of 83 Cancri (6)			— 10 20·9
Tues	2	14 28 14 52	Sidereal Time at Mean Noon, 18h. 48m. 54·28s. Occultation of α Leonis (1½) Reappearance of ditto			10 17·0
Wed	3	13 12 13 42	Occultation of c Leonis (5) Reappearance of ditto			10 13·1
Thur	4		Sun's Meridian Passage 5m. 23·22s. after Mean Noon	2nd Tr. I.	18 52	10 9·2
Fri	5	14 40 15 10	Occultation of 28 Virginis (6) Reappearance of ditto			10 5·2
Sat	6	2 17	☾ Moon's Last Quarter			10 1·3
Sun	7	15 8	Occultation reappearance of B.A.C 4739 (6½)			9 57·4
Mon	8			1st Tr. E.	18 17	9 53·4
Tues	9	3 9	Conjunction of Jupiter and Venus 0° 34′ N. Conjunction of Moon and Mars 5° 38′ N.			9 49·5
Wed	10		Saturn's Ring: Major axis=36″·09 Minor axis=4″·74			9 45·6
Thur	11	7 12	Conjunction of Moon and Jupiter 5° 29′ N. Conjunction of Moon and Venus 5° 54′ N.			9 41·6
Fri	12					9 37·7
Sat	13			2nd Oc. R.	19 34	9 33·8
Sun	14	1 27	● New Moon			9 29·8
Mon	15	14	Conjunction of Moon and Mercury 3° 13′ N. Illuminated portion of disc of Venus=0·890 Illuminated portion of disc of Mars=0·932	1st Tr. I. 1st Sh. E.	18 22 19 34	9 25·9
Tues	16		Sidereal Time at Mean Noon 19h. 44m. 6·09s.			9 21·9

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Capella.
Wed	17	14	Conjunction of Moon and Saturn 1° 20′ S.			9 18·0
Thur	18		Sun's Meridian Passage 10m. 49·92s. after Mean Noon			9 14·1
Fri	19					9 10·2
Sat	20			2nd Ec. D.	18 6 55	9 6·2
Sun	21	21	Neptune at quadrature with Sun			9 2·3
Mon	22	3 53	☽ Moon's First Quarter	1st Sh. I.	19 13	8 58·4
Tues	23	4 53 5 1	Occultation of ϵ Arietis (4½) Reappearance of ditto	3rd Ec. D. 1st Oc. R.	18 45 19 19 26	8 54·4
Wed	24					8 50·5
Thur	25	14 3 14 22 4 8	Occultation of B.A.C. 1648 (6½) Reappearance of ditto Conjunction of Venus with ν^1 Sagittarii 0° 10′ S. Conjunction of Venus with ν^2 Sagittarii 0° 6′ S.			8 46·5
Fri	26	7 11	Conjunction of Mars with ω Ophiuchi 0° 6′ N. Inferior conjunction of Mercury and Sun			8 42·6
Sat	27	17 17 18 6	Occultation of κ Geminorum (3½) Reappearance of ditto			8 38·7
Sun	28	20 38	○ Full Moon			8 34·7
Mon	29	18 11 18 58	Occultation of Uranus Reappearance of ditto	2nd Sh. E. 2nd Tr. E.	17 36 19 24	8 30·8
Tues	30	8 21 9 15 10 51 11 57 12 39 13 28	Occultation of 45 Leonis (6) Reappearance of ditto Occultation of ρ Leonis (4) Reappearance of ditto Occultation of 49 Leonis (6) Reappearance of ditto Saturn's Ring : Major axis=35″·44 Minor axis=4″·03	1st Ec. D.	18 18 15	8 26·9
Wed	31			1st Sh. E. 1st Tr. E	17 49 18 45	8 22·9
FEB. Thur	1					8 19·0

THE PLANETS FOR JANUARY.

At Transit over the Meridian of Greenwich.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	19 58 16	S. 22 40	5″·6	1 13·1
	9th	20 43 50	S. 19 7½	6″·4	1 27·1
	17th	21 4 22	S. 15 39½	8″·2	1 16·1
	25th	20 42 2	S. 14 56	10″·0	0 22·4
Venus ...	1st	16 42 10	S. 20 56	12″·0	21 53·6
	9th	17 24 33	S. 22 13½	11″·8	22 4·4
	17th	18 7 36	S. 22 49½	11″·4	22 15·9
	25th	18 50 51	S. 22 41	11″·2	22 27·6
Mars ...	24th	16 20 50	S. 21 7	6″·0	20 1·9
Uranus ...	1st	9 47 11	N. 14 9½	4″·2	14 59·8
	27th	9 45 6	N. 14 21	4″·2	13 54·8
Neptune ...	1st	2 3 52	N. 10 40½	...	7 17·7
	17th	2 3 45	N. 10 41	...	6 14·7

Mercury may be observed for about an hour after sunset, at the beginning of the month, the interval increasing till the 11th, from which day the interval decreases.

Venus rises two hours and half before the sun on the 1st, the interval decreasing to about one hour at the end of the month.

Mars rises about four hours before the sun.

DISTRIBUTION OF THE BRIGHTER VARIABLE STARS IN REFERENCE TO THE MILKY WAY.

The new star in Cygnus, first seen by Schmidt on Nov. 24, 1876, being again one near the Galaxy, it may for some readers not be without interest to know the distribution of the "Novae," and also of the brighter Variables in reference to the Milky Way. The following list gives the galactic longitudes and latitudes of the "Novae," and of all or nearly all those known Variables, which are or may become visible to the naked eye, and the variability of which amounts to at least one magnitude. The choice being somewhat arbitrary, readers may please themselves to leave some of the following stars out of consideration. The ascending node of the galactic central track on the equator of 1880 is assumed to be in R.A. 18h. 40m., or 280° and the inclination to be 60°. The stars are arranged in the order of their galactic longitudes, which are reckoned from the node.

Galactic.		Star.			1880·0	
long.	lat.		Max.	Min.	R.A.	P.D.
°	°		m.	m.	h. m. s.	° ′
7·4	+42·7	T Coronae 1866 ...	2	11	15 29 37	62 53
8·9	—14·1	η Aquilæ	3·3	4·7	19 46 22	89 18
9·5	+39·8	R Coronæ	6·0	13	15 43 38	61 38
30·1	+14·8	β Lyræ	3·5	4·5	18 45 39	56 47
30·9	+ 1·1	Nova of 1670 ...	3	?	19 42 39	62 59
31·3	+43·9	g Herculis	4·9	6·2	16 24 42	47 51
36·0	+ 3·5	χ Cygni (1686) ...	4	13	19 46 8	57 23
41·1	—70·0	R Aquarii	5·8	<10·5	23 37 37	105 57
43·3	+ 1·9	P Cygni 1600 ...	3	<6	20 13 22	52 20
44·1	— 5·0	T Cygni	5	6	20 42 24	56 4

57·8	— 6·4	Nova of 1876	3	?	21	37	0	47	42
68·0	+ 5·9	μ Cephei	4	5	21	39	50	31	46
72·7	+ 2·3	δ Cephei	3·7	4·9	22	24	43	32	12
82·4	— 8·6	R Cassiopeæ	4·8	<12	23	52	19	39	17
87·6	+ 3·6	Nova of 1572	>1	?	0	18	9	26	31
105·3	+46·8	R Ursæ Maj.	6	12	10	36	9	20	36
116·6	—12·4	Algol	2·3	4·0	3	0	22	49	30
130·4	+ 3·7	ε Aurigæ	3·5	4·5	4	53	22	46	21
134·3	—55·5	*o* Ceti (1596)	1·7	9·5	2	13	17	93	31
180·6	—30·1	R Leporis	6	9	4	54	8	104	59
193·7	+43·9	R Leonis	5·3	10	9	41	6	78	2
255·1	— 2·5	η Argus	1	6	10	40	25	149	3
282·4	+36·3	R Hydræ (1704)	4	11	13	23	10	112	40
315·8	+40·1	δ Libræ	4·9	6·0	14	54	34	98	2
320·0	+17·1	T Scorpii 1860	7	<10	16	9	54	112	41
328·8	— 2·0	X Sagittarii	4	6	17	40	0	117	47
329·3	— 6·2	W Sagittarii	5	6·5	17	57	22	119	35
332·0	+ 4·7	Nova of 1604	>1	?	17	23	27	111	23
334·8	+15·7	Nova of 1848	5	13	16	52	47	102	42
341·0	+51·7	S Virginis	6	<11	13	26	44	96	35
351·7	+44·7	R Serpentis	5·7	<11	15	45	10	74	30
355·1	— 3·1	R Scuti	4·7	9	18	41	4	95	50

66, Lambeth Road, S.E.

A. MARTH.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN JANUARY, 1877.

By W. R. Birt, F.R.A.S., F.M.S.

Fracastorius *the west border.* In the lists for November and December, 1876, attention has been solicited to two lines of mountains and craters nearly at right angles to each other; that given in November being suitable for a base line in laying down a plan of Fracastorius, and that given in December indicating a line of eruption from the crater Rosse on the Mare Nectaris to the south-east angle (?) of Fracastorius. During this month, January, the neighbourhood of the northern end of the west border may form a suitable area for observation, especially as it includes a line of eruption nearly parallel with the line given in November.

Commencing on the west, the first object we meet with is a craterlet, XV. of the synopsis, near the north end of the west wall, it is *ω* of Neison map XIX. Immediately east of this craterlet is a ridge XXIV. of the synopsis extending from the west wall in a north-westwardly direction, it is shown by Neison, Gaudibert and Simms. The next object to the east is a craterlet XVI. of the synopsis shown by Neison and Simms as such, but recorded by Elger, September 30, 1870 as a mountain. Still further eastward, or rather E.N.E. are two more craterlets in a line with XV., XXIV., and XVI., viz. XXVI. a craterlet on the southern extremity of the ridge XXV. which extends from Fracastorius to beyond Rosse. The next craterlet XXVIII. on this line towards E.N.E. is that aligning with Rosse, the crater *b* and the mountain XII. (see list for December, 1876, p. 297). The last craterlet XXIX. the sixth in this line, which is gently curved, the convexity towards the north is situated northward of the shallow crater XI.

The craterlets XXVII. and XXIX. are situated, according to Simms, on a lower level than that of the general floor of Fracastorius. In a letter dated August 20, 1876, he says, "On the evening of July 26, at

8h. 45m. p.m. (about 13h.46m. G.M.T.) I had a good view of Fracastorius, terminator just east of 'Kant,' and bisecting 'Aristoteles.' Arrow-head mountain [XII.] distinctly seen as a separate object. I carefully examined the openings east and west of it, and I feel sure I detected a slight shading occupying the positions XXX. of the two wide bays [formerly mentioned]. I am of opinion that the general surface north of the Arrow-head mountain [XII.], and enclosed by [Rosse, the ridge XXV., the craterlet XXVI., and the shallow crater XI.] is at a lower level than the interior of Fracastorius, and that the sloping ground from the higher to the lower surface occupies the position of the dotted lines in my sketch, thus giving the appearance of two wide bays when seen under a very oblique illumination: the surface also to the westward of the ridge [XXV.] is at a higher level than the low level area before mentioned." Of the *confirmation* of so important an observation as bearing on the selenology of Fracastorius there can be no question.

Erratum.—In the list for November, p. 271, line 8 for *X.* read *XI.*

In the *Monthly Notices* R.A.S., Vol. XXXVII. pp. 12, 13, the following errors occur in "Observations of the Lunar Eclipse, September 3, 1876." At 7·50 "Mons Harpathus" instead of the ring plain *Harpalus;* also "Sinus Proris" instead of *Sinus Roris.* At 9·45 "Harpathus" instead of *Harpalus.* "Senopidus" instead of *Œnopides,* and "crater Agarum" instead of *Prom. Agarum.*

Book received.—"Measurement of the Sun's Distance." By John Harris. London: N. Trübner & Co. 1876.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Aug., 1876.
Dawson, A.

To Dec., 1876.
Bridson, J.
Common, A.
Davies, Rev. R. P.
De la Rue, W.
Escombe, R.
Glaisher, J.
Gould, Rev. R. J.
Green, G. W.
Hollis, H. W.
Huggins, Dr.
Locke, W.
Mestayer, R.
Slater, J.

To June, 1877.
Baron, Rev. J.
Franks, W. S.
Ryves, E. W.

To Aug., 1877.
Guyon, G. F.

To Dec. 1877.
Clermont, Lord.
Gilby, J.
Hall, Rev. R.
Johnson, Rev. S. J.
Joynson, J.
Lambert, O.
Monkhoven, Dr. D. van.
Prince, C. L.
Rogerson, G. R.
Thomson, Prof.
Tidmarsh, Rev. J. B.

TO CORRESPONDENTS.

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All Letters requiring an answer must enclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or *penny* postage stamps, but the Editor will not be liable for loss in transmission.

Post Office Orders for *the Editor* are to be made payable to JOHN C. JACKSON, at Lower Clapton, London, E.

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The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. JACKSON, *Clarence Road, Clapton, E.*, **not later than the 15th of the Month.**

The Astronomical Register.

No. 170. FEBRUARY. 1877.

ROYAL ASTRONOMICAL SOCIETY.

Session 1876—77.

Last ordinary Meeting of the Session, January 12th, 1877.

William Huggins, Esq., D.C.L., LL.D., &c., *President*, in the Chair.

Secretaries—Mr. Dunkin and Mr. Ranyard.

The minutes of the preceding Meeting were read and confirmed.

Mr. Ranyard announced that 45 presents had been received since the last meeting of the Society. Amongst these was a model of the lunar crater Copernicus, which has been presented to the Society by the brother of the late Mr. Bird, of Birmingham. The model is about three feet in diameter, and takes in the mountains Stadius and Eratosthenes, and the other lunar features for some 10° around the great crater. The model will probably be hung in the meeting room, opposite to the model of Copernicus already in the possession of the Society.

The President drew attention to an early telescope which had been presented to the Society by Mr. Bailey. There is no date upon the instrument, but as it is not achromatic and the object-glass is smaller than the eye-piece, it is probable that it dates at least from the beginning of the last century.

The following candidates for the Fellowship of the Society were balloted for and duly elected:

Robert John Baillie, Esq., 10, Bolton Place, Carlisle.
Henry Vere Barclay, Esq., 6, Disraeli Terrace, Putney.
Rev. Daniel Dutton, Sandbach, Cheshire.

Samuel Heywood, Esq., 171, Stanhope Street, Hampstead Road.
Louis Stromeyer Little, M.D., B.A., F.R.C.S., 18, Park Street, Grosvenor Square.
Richard Pearce, Esq., Church Court Chambers, Old Jewry.
Commander William James Lloyd Wharton, R.N., H. M. Surveying Ship "Fawn."
Jesse Young, Esq., Stibbington House, Wansford, Hants.

The President: The present is the time at which according to our bye-laws it is necessary to appoint Auditors to examine the accounts of the Society. It would be more satisfactory if instead of names being suggested from the table the names of three gentlemen were proposed by some Fellow in the body of the meeting.

Mr. Bidder: It has been suggested to me that I should undertake to propose the Auditors, and I believe it is usual to propose the names of some gentlemen who have acted before, as well as some fresh ones. I, therefore, propose Mr. J. Kennedy Esdaile, Mr. A. Brewin, and Mr. F. Barrow.

Mr. Neison: I have great pleasure in seconding that proposal.

The President put the motion and it was carried unanimously.

Mr. Dunkin: Mr. Marth has sent a paper containing an ephemeris for determining the positions of the satellites of Uranus for the year 1877. The paper is composed entirely of figures, and is a very important one to observers who wish to identify the satellites. I have on previous occasions said that we are much indebted to Mr. Marth for the trouble he has taken in computing these ephemerides. Every one who has to do with calculations of this kind must know that great labour is required to produce a table like this, and therefore the Fellows of this Society ought, I think, to feel deeply indebted to Mr. Marth for the contributions he gives to the Society. There is no occasion to read any of the tabular matter in the paper, because it will all be printed.

The President: I need not ask you to return your thanks to Mr. Marth for this and similar papers, which occupy little space when printed, but which require considerable time to prepare, and are invaluable to observers. (Hear, hear.)

Mr. Dunkin: The next paper is from the Astronomer-Royal, and contains observations of occultations of stars by the moon, and of the phenomena of Jupiter's satellites made during the past year at Greenwich. This is a communication the Astronomer-Royal generally makes every year, and though it contains part of the routine work of the Observatory, it is interesting to the Society, and will appear earlier in our *Notices* than in the volume of Greenwich observations for 1876. During the past year, as everybody knows, the weather has been very bad for observations, as it was also in 1875. During 1876 we had only seven observations of occultations, and very few indeed of the phenomena of Jupiter's satellites. These observations are included between February and November, but I may state that though we have observed only seven occultations we have looked for at least 50. As far as the phenomena of Jupiter's satellites are concerned the smallness in the number of the observations is accounted for by the low declination of Jupiter at the present time, so that it is impossible to make observations satisfactorily. Now is the time for the southern observatories to take the matter up, and we shall do so again some five or six years hence.

Mr. Neison: I congratulate you on having seen so many occultations.

Mr. Dunkin: I am happy to say Mr. Knott, one of our Council and an old member of our Society, has contributed an important paper containing a very great number of micrometrical measures of double stars. It is a paper which must have cost him a great amount of trouble—first, in making so many observations and measures, and then in reducing them into shape. Mr. Knott has told me that the trouble of the computations was a great deal more than he was previously aware of. It is an important paper, and one which I hope will appear in our memoirs.

The President: I will ask you to vote the special thanks of the Society for this contribution, which is not only valuable as containing a large number of observations, but because we know we can depend upon Mr. Knott's accuracy. (Hear, hear.)

Mr. Dunkin: We have also received a very important paper from Professor Harkness, of the United States Navy, who is attached to the Washington Observatory, the paper I hope will appear in the memoirs at some future time. In the meantime I will read an abstract. The title is *On the theory of the horizontal photoheliograph, including its application to the determination of the solar parallax by means of transits of Venus.* The instrument described in the paper consists of a heliostat and a long focussed object-glass. The photographs are taken in the principal focus, and there is little or no distortion, such as is to be met with in photographs taken with the Kew heliograph, where the pictures are taken in an enlarged image. When the sun's centre coincides with the centre of the field, the allowance to be made for this distortion is troublesome. But when the centre of the sun's image does not coincide with the centre of the field the calculations necessary to deduce the actual distance between any two points upon the sun's disc, from their apparent distance as measured on the distorted photograph, become very complicated. In making use of the horizontal photoheliograph a glass plate ruled with cross lines is placed over the sensitive plate during exposure, so that any shrinkage of the collodion film can be accurately determined, and a plumb-line is suspended so that it can be photographed on the plates, and affords a fiducial line from which the north point and other angles of position may be determined. The plumb-line is so placed that its intersection with the middle horizon line shows the centre of the plate. The paper, which is of some length, was sent to the Astronomer-Royal, who has passed it on to the Society with the recommendation that it should be printed in our memoirs.

Mr. Brett read a paper *On the specular reflection hypothesis and its bearing on the transit of Venus.* He said: The ring of light which has been observed round the body of the planet Venus in transit has not, as far as I know, been explained; and I propose to show that the specular reflection hypothesis has an important bearing on this subject. If in addition to a burnished reflecting surface we suppose Venus to have a translucent envelope of great density, all the elements for a complete explanation of the mystery are at hand, or at all events it is possible to produce a very similar phenomenon experimentally. In May last I suggested a reason for believing the surface of the planet to be in a molten or vitrified state, and that suggestion was so fortunate as to attract the attention of Mr. Christie, whose investigations afford invaluable data for studying the subject, if they do not afford a complete confirmation of the hypothesis. The theory of specular reflection is well understood. Its phenomena in the case of Venus and the Sun would occur as follows: At superior conjunction we should see merely a small circular image of the sun. In the course of the planet's progress in its orbit this image would move in longitude and extend in latitude, assuming a cusped crescent form. At inferior conjunction it would in approaching the limb present an extremely attenuated rim of light which at transit would completely encircle the disc, and theoretically would never disappear. Thus practically since the light near the limb of the sun is so much weaker than at the centre, it is conceivable that the ring of the reflected image might be observed during the earliest and latest stages of the transit. The position which this ring would occupy within the planet's limb can be determined by a calculation based on the relative diameters of the sun and planet. So far the theory is simple, and has been so well understood for ages that no illustration is needed. It will, however, be perceived that any expansion of this attenuated ring observed at transit would introduce other and more complicated considerations, since it would indicate the existence of a refracting envelope or atmosphere. Now, any diffusion or extension of the area of the ring would weaken the intensity of its light, not to speak of the absorption which would take place. It is comparatively easy to understand notwithstanding these considerations how the line of light may be seen before contact, but the great difficulty arises in finding a diffused ring of light surrounding the planet, when actually on the sun's disc, surpassing in brilliancy the direct light of the sun. There are perhaps theoretical objections to the planet's surface being in a state of fusion, but at all events it may have so existed, and it is not unreasonable to suppose that the metallic constituents of its substance would settle at a lower level than its vitreous elements. We may imagine the surface thus overlaid or protected by an envelope of glass, and outside this an atmosphere. The vitreous envelope would by its lenticular action correct the divergence of the rays after reflection from a zone of the planet's surface immediately beyond the limb condensing them into a luminous halo of greater brilliancy than the sun itself around the planet. This little instrument shows that such a result can be experimentally produced. It consists of a small globe of mercury representing Venus, suspended within a tube. Towards the lamp is a collimating lens, which causes the rays of the lamp, when properly placed, to pass in a parallel beam past the limb of the little globe. On looking in at the eye-piece one sees a black disc surrounded by a ring of intense light. This seems to me to offer a fair representation of the actual condition of the planet. [*At superior conjunction of Venus we should see merely a small circular image of the sun, such as that which has been described by Mr. Christie. In the course of the planet's progress in its orbit the image will become longer and narrower. At greatest elongation it will be crescent shaped, and as the planet approaches superior conjunction the bright patch will extend further and further in latitude upon Venus till at the time of superior conjunction it will be represented by a ring of light all round the body of the planet. No other assumption will, I believe, account for the bright ring of light round the disc of the planet, which has been described by so many observers.] I ask leave only to add one more suggestion and that is that at the next transit it would be worth while for some one with a good telescope and Dawes diaphragm to look at the centre of Venus's disc for the reflection of the earth—for if the envelope has great refractive power I think it not improbable that the image of the earth may be seen reflected there as a minute nebulous speck of light. (Laughter.)

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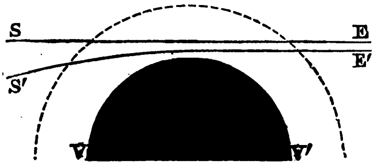
* The paragraph in brackets [ ] was struck out by Mr. Brett when the proof of the report of the meeting was submitted to him for correction.

Mr. Ranyard said: I will, if the Society will permit me, endeavour to explain how the bright ring of light around Venus may be accounted for, without adopting the theory of specular reflection. There can, I think, be no doubt that such a bright ring was seen by several of the observers. And first, as to what has been actually observed just before the planet enters on the sun's disc, a very brilliant ring of light has been seen around the dark limb of Venus which is furthest from the sun. And again, when the planet is seen projected upon the disc, the photospheric light immediately around the dark patch which represents the planet is seen to be a little brighter than the rest of the sun's disc; this is a phenomenon which is clearly seen in the photographs. And without going into the question as to whether the ring of light seen around the planet's disc when on the sun exactly corresponds with that seen around the limb just before external contact, I should like to explain how such a ring may be accounted for without making the rather extraordinary supposition which Mr. Brett has suggested to us that Venus has a vitreous envelope. First let us consider the case when the planet is just off the sun's limb. If A B in the figure be the body of the planet, and S E the course of a ray through the upper part of the planet's atmosphere, we should expect to find its direction comparatively little altered by refraction as it passed through the atmosphere of the planet, while a ray passing lower down through the atmosphere would be considerably bent. If the refraction of the atmosphere of Venus were equal to that of the earth's atmosphere a ray passing low down so as nearly to graze the limb of the planet, would be refracted so as to make an angle of more than 1° with its former course, an observer in the direction E looking at the further limb of the planet before it comes up to the sun's limb, would at a certain height in the planet's atmosphere corresponding to the height where the deviation of the ray is sufficient to bring the sun's nearest limb into view, see a brilliant point of light corresponding to the sun's nearest limb. Lower down in the atmosphere of the planet the deviations of the ray will be greater, and more distant parts of the sun's body will be seen refracted through the planet's atmosphere. Thus, say some ten or twenty miles below the height where the upper limb of the sun is brought into view by refraction we should have the density of the planet's atmosphere sufficient to bring the lower or more



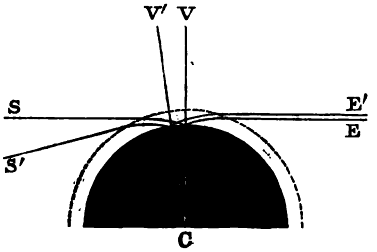
distant limb of the sun into view. As seen from the earth the interval between these two altitudes would appear brilliantly illuminated with the sun's transmitted rays, and the observer would see a narrow line of light surrounding the limb of the planet furthest from the sun's disc, and since the light from the whole disc of the sun is compressed into this narrow area, we should expect to find the bright line (unless the absorption in Venus's atmosphere were very great) brighter than the sun's disc.

Without, therefore, making the extraordinary assumption of Mr. Brett, as to a vitreous envelope encasing the planet, it is possible to account for the observed phenomenon. The course of the ray in the glass envelope of the thermometer would not correspond to the course of the ray in the planet's atmosphere, for a ray which entered the glass envelope of the thermometer bulb would be reflected backwards and forwards from one surface to the other, so as to encroach upon and illuminate the dark hemisphere, but I need hardly say that we cannot conceive of such reflection from within the upper surface of the atmosphere of Venus, for the intensity of a reflected ray depends, as is well-known, upon the difference of the refractive indices of the two media on either side of the bounding surface from which the reflection takes place, and in this instance the intensity of the reflected ray would be almost *nil.*

My friend Mr. Brett seemed to think that as Venus passes from opposition to greatest elongation, the spot of light which would be seen (supposing there to be specular reflection from its surface) would have become longer and longer till at greatest elongation it would assume a crescent form, and so onward to the annulus; but I need hardly occupy the time of the Society in pointing out that the patch of reflected light from a sphere would always occupy the same length in latitude, though its apparent breadth as seen from the earth might diminish in longitude; the contour of the patch upon the planet would in fact always be a circle.

Capt. Noble: If I understand this theory of specular reflection, and I really hardly can believe I do, Mr. Brett contends that the planet Venus is like a polished globe of quicksilver, surrounded by a shell of glass. It may of course be possible that Venus is a huge thermometer bulb hung up in the heavens, but we shall require a great deal of evidence to make us admit such a theory; and I should like to ask if the atmosphere be vitreous, how are the dark markings which have so frequently been observed appearing and again disappearing on the surface of the planet to be accounted for?

Mr. Christie: I am sorry to find there is some misconception of what specular reflection means, and what its laws are. Such strange statements have been made on the subject that I feel bound to try and attempt some explanation. I think this point of Mr. Brett is one of very great importance. I confess it did not occur to me at first sight that specular reflection on the opposite side of Venus could ever cause the light seen round the planet to appear brighter than the disc of the sun itself; but on looking at the matter a little more closely, I clearly saw that it would be so, from the ordinary laws of optics. I will attempt to give a slight explanation because there seems to be a mystification about this. Disregarding the hypothesis about a vitreous atmosphere, which is not essential, though it is quite possible, we will consider the rays as reflected at the surface of the planet and then refracted.



If S E be a ray from the north limb of the sun, and C V be a normal to the surface at the place where the ray is reflected, we shall have the angle of incidence equal to the angle of reflection, and the course of the ray after it has been reflected at the surface of the planet and refracted in passing out of the atmosphere may be found. A ray from S′, the south limb of the sun, must, in order to reach the observer on the earth, be reflected at another part of the surface, being afterwards refracted so as to be parallel to the former ray, and for this the only condition that has to be satisfied is that the angle V C V′ should be equal to half the diameter of the sun as seen from Venus. When this condition is satisfied the rays from opposite limbs of the sun will be parallel on emerging from the planet's atmosphere. The diameter of the sun, as seen from Venus, is say 50′, and, therefore, the angle V C V′ will be about 25′, and, consequently, looking from the earth we should see the whole light of the sun condensed into a narrow zone 25′ broad, as measured from the centre of Venus, and consequently the light from the sun would be very much condensed; so that actually more light may in this way be received from Venus by specular reflection than is received directly from the sun, notwithstanding the absorption that takes place in the atmosphere of the planet. This seems to me to account for the fact, if it is a fact, that there is a bright ring of light round Venus brighter than the sun, notwithstanding the absorption of the atmosphere. It is the action of this convex mirror, which condenses the rays and makes the smaller image brighter, that would, as I understand, explain the point. The bright ring has been seen by certain observers; they have described it as a bright ring, distinct from the light of the sun, which, therefore, could not have greatly overpowered it, and they distinguished it only by a difference of tint or hue. There are on the other hand observers who did not see it, but that of course is easily explained, as unless the eye is trained to distinguish varieties of tint such a thing might be easily passed over. I have myself found that in this specular reflection of Venus the central spot is nine times as bright as the limb; but if I had examined it casually only, I should have said there was no difference of brightness. I may add that I think negative evidence, however abundant, should never be allowed to destroy positive evidence. There is just one other point, viz.:—supposing you see a bright spot on Venus at a certain part of the disc, the question is whether that is to be referred to specular reflection or to mere ordinary scattered reflection. Of course scattered reflection would be at a maximum directly under the sun at the point where the sun is in the zenith, whereas the maximum for specular reflection would be at the point where the sun is at the same distance from the zenith as the earth is. With reference to the observations I made on Venus, when I made them the planet was slightly gibbous. Now, if the patch of light had arisen from ordinary diffused reflection it would have fallen very close to the limb, practically on the limb itself, but if it arose from specular reflection the point would have been nearly half-way between the limb and the terminator, and I must say myself that that was as near as possible the place where I observed it, that is, about the middle of the disc. The question whether there is a bright spot is another matter. There would be a bright spot on either hypothesis, but the bright spot from ordinary diffused reflection would be close to the limb, hardly distinguishable from the limb. When the planet is at quadrature with specular reflection, the spot of light would appear to be three-tenths of the angular radius from the limb. And at the same time the point of specular reflection would be very much brighter than any part around it; and the ratio of brightness is also a matter to be taken into consideration, for with diffused reflection the gradation would be comparatively slight. I mention these points because they should all be borne in mind. There is a great deal to be worked out on the subject, and I should be sorry to lay down any positive statements one way or the other. I have simply given you my observations as I made them. It is not sufficient to say there is a bright spot, but we must know the exact position; and if the brightness can be accounted for on any other hypothesis it would of course be worthy of consideration, and I hope there are observers who will test the point.

Mr. De la Rue: Is this explanation to apply also to the bright line on the sun's disc, visible at the time of a partial eclipse round the moon's limb?

Mr. Christie: I am really hardly prepared to answer that.

Mr. De la Rue: Because it is desirable to include the two cases in any explanation of the phenomena.

Mr. Christie: I can quite believe you must include the two, but it is hardly fair to expect one to include the two at a moment's notice. (Laughter.) When first this hypothesis of specular reflection was presented to me I thought that it would not account for the facts, and I did not see how to explain them, but on thinking it over I saw my way, though I am quite open to correction on the point. I do not think I could explain the other matter at the moment, because, of course, the difficulty of the point is that you must bring the rays into parallelism so that the eye may receive them, and for that purpose an atmosphere is wanted, and I am far from admitting that there can be an atmosphere of appreciable density round the moon. At all events we could not admit an atmosphere which would give a horizontal refraction of half a degree.

Mr. De la Rue: Moreover the surface of the moon generally could scarcely be assumed to be capable of specular reflection.

Mr. Christie: I am not quite certain that I understand Mr. De la Rue. He raises the point of the bright line round the moon. A bright band has been seen in front of the advancing limb of the moon, but it strikes me that is rather to be attributed to an effect of contrast. Whether it is so I am not prepared to say, but it might be explained in that way; at the same time I do not see how it is to be referred to the same cause as the bright ring round Venus, which is of a different character.

Mr. De la Rue: It has the same appearance.

Mr. Neison said, that there was certainly nothing like specular reflection from the surface of the moon; he questioned whether Mr. Christie's explanation would be possible without supposing the existence of a much greater horizontal refraction than he had assumed. Refraction through an atmosphere seemed a simpler view.

Mr. Christie said the drawing was of course rough, but his figures, he believed, would correctly explain what takes place.

Mr. Bidder: I have been listening to Mr. Christie, and I am not quite sure whether he believes in specular reflection or not; but I want to ask him a question. I wantto know whether it is necessary for the explanation of the bright line to assume any specular reflection from the surface of Venus at all. Let me suppose Mr. Christie's circle representing Venus, to represent the surface of her atmosphere; taking the two rays he has drawn, the one from the north limb of the sun and the lower one from the south limb of the sun,—then, is not this the case, that the atmosphere necessarily acting as a lens, the lower, or south, ray which makes the greater angle, will be more refracted at the surface of the atmosphere of Venus than the upper ray, and thus the effect would be to make these two rays more nearly parallel, and so to condense the light passing through the atmosphere? If that is so, you have in the lenticular action of the atmosphere, without supposing specular reflection, sufficient to account for the condensation of light producing the bright ring of light round the edge of the planet.

Mr. Ranyard said: I should like to point out that, according to Mr. Christie's view, if there is reflection from the surface of the planet, and then refraction in an atmosphere there would be two zones of light, one within the other. For there would certainly be a zone of light visible from the earth due to the rays which had only passed through the atmosphere of Venus at different levels, as I before explained, and the zone due to the transmitted rays would certainly be much the brightest of the two, for in its case the light would only be lost by absorption in the upper atmosphere, while in the course of the ray described by Mr. Christie there would not only be the loss of light caused by passing through the whole depth of the atmosphere, before and after reflection, but there would also be the loss of light at reflection, the reflected ray being as we know always less intense than the incident ray. As seen from the earth no doubt the two zones would appear merged in one another, but if for a moment I admit Mr. Christie's hypothesis, I must point out that my zone would also exist and be much brighter than Mr. Christie's zone, even on the supposition that the body of Venus reflects light like a polished mirror.

With regard to the line of light which has been spoken of as existing round the moon's limb when seen upon the sun, there can be no doubt at all as to its presence on the photographs of partial phase eclipses. The line has been observed over and over again. In 1860 the Astronomer-Royal and Mr. De la Rue both noticed it, and wrote papers in the *Monthly Notices* upon the subject. They considered, as far as I remember, that it was due to a sense of contrast. Mr. De la Rue or the Astronomer-Royal, I forget which, cut out from a paper photograph of one of the partial phases a circular disc of paper from within the limb of the moon, and he then placed this disc of dark paper upon another solar photograph, and thought, if my memory serves me, that he saw a bright line bordering the edge of the imitation moon, which line of light must of course have been due to contrast. Professor Stephen Alexander, Professor Challis, and Professor Morton, all made experiments, and they all agree as to the fact that a band of increased brightness is to be seen on the sun's disc bordering the limb of the moon. In 1869 still stronger evidence of the actual existence of the bright band was found, for Dr. Curtiss experimented on some of the partial phase photographs which had been taken at Des Moines, and found that when he placed the negatives over a piece of paper on which there were dots of various degrees of blackness that, on moving the negative over the paper, the increased opacity of the collodion film at the part corresponding to the bright band was such that dots could not be seen through it which were distinctly visible through the collodion film at other parts of the sun's disc. I ought to mention, too, that a similar bright band is to be seen if carefully looked for on every over-exposed photograph around every dark object that abuts on a bright field, such a bright band is distinctly to be seen—for example, in the photograph of the Oxford Observatory, which is given as the frontispiece to the last volume of the *Monthly Notices*. I think it will be found, though I have not made any experiments which would warrant me in speaking positively, that the line of light has its origin in some circulation which takes place in the liquid film covering the photograph, a circulation which in some way causes an increased photographic action on the parts of the plate adjacent to areas where no photographic action has taken place. But in the case of the photographs of the transit of Venus, I do not wish for a moment to assert that the bright band seen round the image of the planet is only due to this cause, I think that it will be found to be due to the combined effect of the photographic action, and to a ring of light similar to that which is seen round the limb of the planet before contact. My reason for believing in the actual existence of the bright ring round Venus when on the disc is that we do not find in nature discontinuous phenomena, and as the ring is seen before and after the planet is upon the sun's disc, no doubt it is also to be seen when the planet is upon the disc.

Capt. Abney: In the photographs of the transit of Venus the bright line is so very simply accounted for, without any theory other than a photographic theory, that I think it might be left out of the discussion, because the bright border is a thing we meet with every day. It is only a photographic phenomenon, and I think too much stress has been laid on it. I have seen some photographs of the sun on a small scale which had a similar band round them, but the person who took it not being a photographer did not understand what it was, and spoke of it as being a photograph of the corona. But those who study photography are perfectly accustomed to these things, and I think the part of the discussion as to what appears in the photographs might be left out.

Capt. Noble: If any one looks at a photograph of the dome of St. Paul's taken against a bright sky, he will see a splendid corona round the dome.

Mr. Brett: Am I, Sir, expected to reply? because I would remark that I have not heard anything to reply to.

The President: I hope by this discussion of the subject we shall eventually get some light upon this rather obscure subject. I do not see anything theoretically impossible in the supposition of specular reflection from Venus. As to one point mentioned by Mr. Brett, viz., that of seeing the reflection of the earth at the centre of Venus. A bright point has been seen at the centre of the planet Mercury during a transit. I saw it myself under circumstances which left no doubt in my mind that it was a real phenomenon, and it has been seen by others. There is a figure or small representation of it in the *Monthly Notices.* I will ask you to give your thanks to Mr. Brett and the various speakers.

Lieut. Neate said he had had a favourable opportunity of observing the transit of Venus, and to him the light round Venus appeared crescent shaped, like a very young and thin moon.

Mr. Dunkin read a paper by Dr. Wentworth Erck *On an improved mode of viewing the sun.* The image from the object-glass is in the first instance enlarged by means of a Barlow lens, and then a small unsilvered glass reflector is placed so as to throw a part of the image upon the eye-piece. With the instrument as used with his own telescope, only one 360th part of the heat of the sun reaches the eye-piece, which is thus kept perfectly cool, and the observer can continue to view the sun with perfect comfort for hours at a time.

Capt. Noble thought that this method of viewing the sun was not novel, but on an explanation from Dr. Erck owned that he misapprehended the description.

The President: If I understand it rightly, this eye-piece has certain advantages over the Dawes' eye-piece. In Mr. Erck's eye-piece only the light from a small portion of the sun is thrown on the diaphragm and lenses of the eye-piece, and the rest of the sun's light and heat passes on into space. Anyone who has used Dawes' eye-piece must know how hot it soon becomes.

Mr. Christie said, I should like to point out that there is an inherent defect in using a reflector in front of the eye-piece, and that is that every defect of the flat surface is magnified, whereas if you place your flat surface behind the eye-piece the defects of the surface are simply transmitted to the eye in their ordinary state, whilst the area necessary is very much smaller, and consequently the flat is less expensive.

Dr. Erck: But if you put the prism where Mr. Christie has suggested the heat on the eye-piece would be very intense. And with regard to the objection as to the defects of the flat surface being magnified when in front of the eye-piece, it will be remembered that this is also the case with the flat of an ordinary reflecting telescope with which high powers are used, and yet no very great difficulty is found to arise. My flat is small. I may remark that the atmosphere is scarcely ever in such a condition as to allow powers higher than 150 or 200 to be used upon the sun, whereas with double stars people use powers up to 600 or 1,000, and find no great inconvenience arise from magnifying the errors of the flat.

The following papers were also announced and partly read:—

A. Marth: *On the Orbit of α Centauri.*

J. I. Plummer: *On the conjunction of Venus and λ Geminorum.*

Cleveland Abbe: *On O. M. Mitchell's Observations at Cincinnati.*

W. F. Denning: *Observations of Shooting Stars, April to December,* 1876.

J. R. Hind: *On the Orbit of α Centauri.*

The meeting adjourned at ten o'clock.

REVIEWS.

Astronomical Lectures. Measurement of the Sun's Distance. By John Harris. N. Trübner & Co., Ludgate Hill, 1876.

Mr. Harris proposes seven methods for measuring the sun's distance. Concerning the first, and others also, we may observe that any method by which the parallax of the sun is to be obtained by direct observation with graduated circles and transit instruments, so that the errors of instruments and observation affect the quantity sought for by their full amount, is of no value. About the second method; the moon's terminator is far too undefined a line for any delicate measurement depending upon it. The method of Aristarchus, well known to many, ingenious as it is, is useless on this account. And whatever ingenuity there may be in Mr. Harris's *brochure*, we fear there is not one of his seven methods that can be considered sound and practicable. We say we fear, because we confess that there is much in this work that we have not been able to comprehend. To give just one instance: on page 17 we read, "let us assume the observed time of the passage of the moon's diameter across the sun's centre, or over the solar spot, to be 1′ 56″." Over this, and much more, we have puzzled in vain. The author does not seem to be aware (for anything at least that appears in this work) that there are five or six independent and reliable methods of obtaining the sun's distance, apart from the transit of Venus, and that all of them very closely coincide in results. We wish we could have bestowed a word of praise on this neatly got up work, and should be sorry to do injustice to its author,—admonished, moreover, by the gold balance figured on the cover, to weigh his production impartially. We remember seeing many years ago in one of the Art Galleries of Italy, a pretty piece of sculpture representing Hope feeding a chimera;—an emblem of extensive application; and which placed on the outside of many a book would form by no means an inappropriate introduction to its contents; for it would be often symbolic at once of the wasted time and money of the writers, and of the disappointed expectations of their readers. In the present case we may console ourselves as to the last mentioned, by the reflection that our own lack of intelligence may be in part the cause. We should be glad if it were to prove so; and also that as regards the author our own apprehensions, and not his lucubrations, should turn out to be chimerical. But we nevertheless are not without misgivings on this head.

Eleventh Report of the Board of Visitors to the Melbourne Observatory; with the Annual Report of the Government Astronomer for the year ending 20th June, 1876. Melbourne.

These reports are in every respect very satisfactory. The great telescope, worked by Mr. Turner, is being devoted almost entirely to the revision of the nebulæ figured by Sir John Herschel, at the Cape of Good Hope in 1835; and this work will be published with as little delay as possible; the inquiries for the results of the observations with this telescope constantly received from Europe and America are numerous and urgent. 70 nebulæ and clusters have been examined; several of the former are considerably changed, and others so completely altered as to be scarcely recognisable save by their position with respect to adjacent stars. No decided changes have been detected in the nebulæ about η Argus since March, 1875. Mr. Ellery has found that the best style of reproducing drawings of nebulæ, &c. is a toned lithograph, *white* on black ground, and the specimens obtained represent the nebulæ as they appear in the telescope more nearly than any other method he has seen.

Out of about 150 nights which were more or less fit for observing with the telescope, 40 were solely occupied with visitors. We are not disposed to regret this; which may be to the advantage of astronomy in many ways.

Mr. Ellery returned to Melbourne from his visit to Europe in April last; the work of the observatory having during his absence been most satisfactorily conducted by Mr. White, the Chief Assistant. The 8-inch equatoreal has been greatly improved, and its performance is now excellent. The results of astronomical observations from 1871 to 1875 are ready for printing, and the meteorological and magnetical work for 1875 nearly so. Unusual pressure of work has been occasioned by the transit of Venus. Drawings of Mars and Jupiter and observations of the conjunctions of Saturn's satellites have been made, and 143 sun-pictures. It is intended to continue the usual observations with the transit circle, which Mr. Ellery considers as the most important and fundamental work of the establishment. According to Sir George Airy, "the Melbourne Observatory has produced the best catalogue of stars of the southern hemisphere ever published." The meteorological and magnetical work is under the charge of Mr. Moerlin. The subject of intercolonial weather telegraphy, to include New Zealand and Tasmania, is receiving much attention. Time is distributed to the Colony from the observatory; 49 chronometers have been rated, and 26 Aneroid barometers tested during the year; besides what has been mentioned, other useful work has been done, and at the close of his report Mr. Ellery says, "During my late visit to Europe I made it my business to inspect the equipment and enquire into the working of as many of the English and Continental observatories as possible. I saw very much to interest me, and gathered important information on many points. I was, however, highly gratified to find that the opinion expressed by the American and German astronomers—who came out to observe the transit of Venus—on the efficient state of the Melbourne Observatory was borne out by personal experience; for out of ten European observatories I visited, with the exception of Greenwich and Paris, I did not see any better or even so well equipped as our own, or one in which a greater amount of standard work is being done. I cannot find that we are much behindhand in any direction, but in some respects the reverse; and the only detail in which I considered the Melbourne Observatory is deficient, as compared to even Greenwich or Paris, is in the small size of our transit circle. Most of the European and American observatories possess, or are now being provided with, instruments of this kind with larger apertures and of a more complete form than the one we possess; not that I believe better work is likely to be done with these instruments than our own, but the larger apertures of course render it possible to obtain observations of much fainter stars than can be got with the smaller."

The Sun: Ruler, Fire, Light, and Life of the Planetary System. By Richard A. Proctor. With nine lithographic plates (seven coloured) and 100 drawings on wood. Third edition. London: Longmans, Green & Co. 1876.

Fortunate are the persons who are commencing their astronomical studies at this late period in the century, with aids both as regards books and instruments undreamed of fifty years ago! Those, like ourselves, who have more or less been occupied with the science during that time, can well appreciate the difference. In this country Mr. Proctor has produced more popular or semi-popular works of value on the subject than any other writer, and it is gratifying to find that they live and improve through successive editions, and that the pains of their indefatigable author are rewarded by an extensive circulation, indicative, we hope, both of an interest in astronomy before existing on the part of many, and of their instrumentality in creating such an interest for the first time. Let us just observe, in passing, that as a sound and altogether trustworthy popular work on astronomy can only be written by a mathematician of adequate strength, so, to become comprehensively speaking an astronomer, a good foundation of mathematics must be laid, and we are hopeful that the reading of Mr. Proctor's books may, on some at least, impress this, whilst those who are more particularly devoted to the observational department, or who are contented, as the great majority must always be, by attaining to an acquaintance with results, will find in them the latest and best information lucidly and attractively displayed.

The first edition of *The Sun* was briefly reviewed in this journal in March, 1871, and it might seem superfluous now to renew our encomiums, nevertheless we cannot refrain from a somewhat more extended notice of this edition, which besides having been carefully revised, has the addition of some important sections, including an account of Prof. Langley's (U. S.) researches. The engraving of the great sun spot of Dec., 1873, from studies made at the Alleghany Observatory, with the equatoreal of 13 in. aperture, by Prof. George F. Barker, of Philadelphia, is the most beautiful illustration of the kind we have ever seen. There is no index, but this want is supplied by a copious table of contents. The headings of the chapters are as follows:—I. The sun's distance and dimensions. II. The sun as ruler. III. Analysing sunlight. IV. Study of the sun's surface. V. The prominences and the sierra. VI. The corona and zodiacal light. VII. Physical condition of the sun. VIII. The sun our fire, light, and life. IX. The sun among his peers. The plates and woodcuts are all excellent. The account of the state of the great problem of the sun's distance, from Aristarchus to the most recent and refined methods, is given in Chapter I. In Chapter II. the sun's influence at all distances, near and far, a subject almost untouched in astronomical books, is discussed in a most elaborate way, and the numerical results are of high interest. The mathematical formulæ belonging to this subject will be welcome to some. The author is in the habit (and it is a good one) of closing each chapter with a summary or resumé, and we should like to have transcribed part of what he has written at the end of this one on the past history of our system, and "of the amazing conflicts in which it had its birth."* The concluding reference to a passage in the book of Job we rejoiced to see, for it worthily closes a grand speculation on the remote past.†

In Chapter III. the author has spared no pains to make the explanation of spectroscopic analysis as simple and at the same time as complete as possible, and we think he has succeeded well. An improved and powerful spectroscope, devised by himself and constructed by Mr. Browning, is described and figured, and the wonderful application of the spectroscope to measure the velocity of recess or approach of stars, &c., is admirably elucidated. This valuable chapter winds up as follows: "By an observation not occupying many seconds, any clear-sighted observer, armed by our opticians with adequate spectroscopic power, can measure the swiftness of the solar wind-storm, can gauge the pressure of the solar atmosphere, and can estimate the relative temperature of spot and faculæ, of photosphere and chromatosphere, and, lastly, of the higher regions to which eruptions cast those masses of glowing vapour which form the solar prominences." In a note, p. 107, the idea is suggested that there may be even terrestrial creatures directly sensible of the action of the shorter waves of light corresponding to parts of the spectrum beyond the violet end, and thus possess an additional sense (to those of touch and of sight) with which human beings are not endowed. This appears very probable, and may perhaps tend to explain the extraordinary and inexplicable faculties of some animals.

Chapter IV. presents as much about the solar surface and spots from the time of Fabricius to the present period, as suffices to give a good general view of the subject, and this epitome formed from a vast mass of material, and itself of some considerable length, must have cost much trouble in preparing. Regarding Fabricius and Galileo, it is surprising that neither of them seem to have thought of the simple device of a piece of smoked glass for viewing the sun. Turning over Grant's *History of Physical Astronomy*, we find he makes a similar remark, p. 227. Galileo inferred that the blackest nucleus of a solar spot is brighter than the brightest part of the full moon. Photometrical experiments by Sir W. Herschel led him to the conclusion that the full light of the sun, the brightness of the penumbra, and that of the nucleus would be represented respectively by 1000, 469, and 7. If we take Zollner's estimate of the brightness of moon and sun as 1 : 618000, the brightness of the nucleus will be 4326 times greater than the general brightness of the moon, and if we assume the brightest part to be twice as bright as the average surface, we find that the nucleus is above 2000 times brighter, for instance than Aristarchus, which abundantly confirms the sagacious conclusion of Galileo. And Prof. Langley says, that "when all extraneous light is excluded, except that from the 'blackest' part of the umbra, this proved to be not only intrinsically bright, but insupportably intense to the naked eye."

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* It may be (faintly) suggested whether the Titanic battle (Hesiod) of Greek Mythology, might not refer to this primeval elementary strife. Mr. Keightley remarks (*Mythology of Ancient Greece and Italy*): "In this narrative of the hiding and destroying of children, the wars of different dynasties, and the other events, we seem to discern a tradition or conception of the gradual progress of creation, and of the violent commotions of nature in a time anterior to the earth's assuming its present state, and becoming obedient to the laws which now regulate it; a truth confirmed every day more and more by the discoveries of geology." But the history of the solar system, and not only the earth itself, may be in question. At any rate when the Titans were overcome and chained in Tartarus, the only members of the race who remained in power were Oceanus (Ocean), Helios (Sun), Selene (Moon), and Eos (Dawn).

† After alluding to the power and works of the Almighty in the invisible world, the earth, and the heavens, the patriarch says, "Lo, these (are) parts of his ways: but what a whisper of a word is heard of Him! but the thunder of his power who can understand?"—Job. xxvi. 14. A close rendering of this beautiful passage. It is a wonderful book.

In Chapter V. on the prominences and the Sierra, with its beautiful illustrations, we are introduced to some of the marvels presented by the sun, such as the vast dimensions of some of the prominences, the terrific violence of the solar storms, how Dr. Huggins first succeeded in seeing the prominences themselves, and the wonderful outburst from the sun observed by Prof. Young, of America, on September 7th, 1871, illustrated by views, the most remarkable ever witnessed by man, when matter was carried from a height of less than 100,000 miles to a height of more than 200,000 miles in ten minutes, and in which Mr. Proctor has calculated that the ejectional velocity exceeded enormously—exceeded *many times*—the utterly inconceivable velocity of 255 miles per second, "It may be concluded," he writes, "that the matter of the smallest jet prominence actually recognisable as such must have been ejected with a velocity sufficient, if there were no atmosphere, to carry the matter for ever away from the sun."

Chapter VI. on the corona and zodiacal light, is of the highest interest, and the subject is treated with the firm and bold yet cautious hand of a master. Converging evidence of all kinds is brought to bear on this wonderful solar appendage; an atmosphere of enormous extent—countless meteor systems—auroras—cometic phenomena—electrical processes—and, finally, ejection of matter from the sun, an hypothesis of the author which he has made to appear very probable. This chapter ends as follows: "The study of the corona has led us to conclusions involving relations of cosmical importance. We have been led to associate the corona with the scheme of meteoric systems, and therefore with the mysterious phenomena presented by comets. We have evidence also associating the solar corona with coronæ surrounding the millions on millions of suns scattered throughout space. In studying the processes which thus seem to be at work, we are led to consider their bearing as well on the past as on the future of the universe of solar systems. A new bond of association is recognised between these uncounted millions of systems. Already light has brought its message from the star depths surrounding us on all sides, and we have learned to believe that the light-record dates from a few years to thousands of years ago. But meteoric visitants from other suns must have occupied millions of years in each voyage from sun to sun, and many have in all probability traversed myriads of such voyages before their existence has been brought to a close on our tiny earth. The past thus revealed to us is to all intents infinite in duration. But processes resembling those which led to these infinitely prolonged migrations, are even now beginning, as fresh meteoric matter is propelled from our sun and his fellow suns. These meteoric outbursts reveal the future to us as no less infinite than the past. We may truly say of time as thus pictured to us, what, in Goethe's wild vision, the Angel said of space : 'End is there none to the universe of God. Lo! also there is no beginning.' "

The difficulties attending the question of the physical nature of the sun are discussed in Chapter VII. Elements exist in the sun in conditions, on account of enormous temperature and high pressure, unlike those we are familiar with; and we are as yet but imperfectly acquainted with certain physical laws, such, for example, as the distinguishing marks of the solid, liquid, and gaseous states; of incandescent solid and fluid bodies ; and the limits of the rarefaction of gases. The probable effects, too, of the amazing velocity with which matter is moving in the sun, "amid depths of vaporised metals, and, for aught we know, over seas and continents of liquified and solidified gases," are beyond our present powers to estimate. And as for combustion, " for aught that is yet known, the intensity of the forces at work upon and within the sun may wholly prevent the occurrence of any processes of combustion familiar to ourselves, while other processes of true combustion, altogether unthought of by us, may be in continual action." The evidence given by the spectroscope is also found to be imperfect beyond a certain point, and "it is far from unlikely—if, indeed, it is not to be regarded as certain—that the visible edge of the solar disc lies considerably above the true limit of the photosphere." The sun-spots also present many perplexing problems. It is still doubtful whether they are formed by forces acting from without or from within. The origin of the prominences is still a mystery, and the corona's true nature also unknown. "Here, as in so many matters relating to the physical constitution of the sun, we must perforce wait until our experimental knowledge and our instrumental means have been very largely increased."

(To be Continued.)

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

ON SPECULAR REFLECTION FROM VENUS.

Sir,—The statement contained in the letter by Captain Noble on this subject in the *Astronomical Register* for January, is so astounding that I can only conclude that the writer does not understand the meaning of the term " specular reflection," which is commonly supposed to imply that the light is reflected from the surface like that of a mirror or speculum. Now, the well-known law in that case is that the angle of reflection is equal to the angle of incidence, and therefore if specular reflection take place from Venus, the normal to the surface at the point of reflection must bisect the angle between the sun's direction (that of the incident light) and the direction of the earth (that of the reflected light). Now, at quadrature the angle between these two directions is 90°, from which it follows that the point at which specular reflection would take place must be 45° (on a great circle of Venus) from the point immediately under the sun, *i.e.*, the limb, and that its apparent distance from the limb for a spectator on the earth must be vers. 45° × angular semi-diameter of Venus, or about three-tenths of the semi-diameter of the planet. A glance at a globe illuminated by a gaslight or a moderatety distant window, will show, independently of all reasoning, that the spot of light reflected from a smooth surface *never* falls actually on the limb, and that at quadrature it is at a considerable distance from the limb. During a transit across the sun's disc the spot would become a ring of light all round the limb.

All this was explained so clearly by Mr. Brett, at the meeting of the Royal Astronomical Society in May, that I thought it hardly possible there could be any misconception on such a simple point, and I feel that an apology is due to your readers for discussing these elementary questions. My excuse must be that the question whether the surface of Venus is molten or not, is one of considerable interest, and it is important that erroneous ideas on the subject should not be allowed to pass uncorrected.

I must confess that a "rudimentary acquaintance with optics" does *not* enable me to recognise the strange law which Capt. Noble appears to have assumed, and by virtue of which light incident perpendicularly on a surface is reflected "specularly," at a grazing angle of 90°! It is, therefore, a satisfaction to me to find that the spot of light which I observed was not in the position in which Capt. Noble's theory of specular reflection would have placed it. Certainly, on this new theory of optics, it may be physically impossible that Mr. Brett can have observed "specular reflection," but when a statement is utterly opposed to all received ideas on the subject, it can hardly be considered a recommendation that it is also at variance with the results obtained by a careful observer, who has the advantage of possessing a more highly trained eye than is usual among astronomers.

Blackheath: Jan. 6, 1877. I am, sir, your obedient servant,

W. H. M. CHRISTIE.

REFLECTION FROM VENUS.

Sir,—After hearing Mr. Brett's astonishing description last night of (what he imagines to be) the physical structure of the planet Venus, I can only say that had I been earlier familiar with it, I could never have employed the expression *specular* reflection in my letter in this month's *Astronomical Register.* My own observations of Venus, extending over a period of twenty years, have satisfied me that she is surrounded by a very dense and extensive atmosphere, indeed, practically obscuring the body of the planet altogether; a fact which I believe to be abundantly corroborated by the degradation of illumination at her terminator; the crepuscular light occasionally visible at her cusps, and the dusky spots which have been seen by De Vico, De la Rue, and others. Believing this still, I see now that the material part of my second paragraph should have stood as follows:—

I would point out that Mr. Christie's observation of a spot of light, was made at a time when Venus was sufficiently gibbous to have brought that spot (supposing it to have had its origin in the dispersion of light from a rough or dead surface) upon the disc of the planet—although not, it would seem, where he placed it: but in Mr. Brett's case, his assumed observation was made when of course the most brightly illuminated part of the planet would be *on* the limb itself, and, *ex necessitate*, could not be visible as a patch of light. The most rudimentary acquaintance with optics will enable any one of your readers to construct a diagram of the sun, earth, and Venus when the latter is at her greatest elongation; and to satisfy himself, by mere inspection, of the impossibility of the bright patch corresponding with the area of greatest illumination being perceptible at that time to a terrestrial observer. Mr. Christie may—or may not—have observed the bright patch I have spoken of, but it is impossible that Mr. Brett can have done so.

But Mr. Brett told us last night, at the Royal Astronomical Society, that Venus is merely a burnished metallic globe, encased in a transparent vitreous one; upon which again is superposed an atmosphere, diaphanous to rays entering it, but, somehow, causing their "diffusion" as they emerge from it. *If* then the planet be nothing but a kind of glorified thermometer-bulb, surrounded by an envelope so curiously constituted as to let the light in, but disperse it as it comes out again; my argument falls to the ground, and I have nothing more to say. I might though, perhaps, submit that a little proof of such utterly abnormal physical structure would seem to be required, before we accept it as an established fact.

I have the honour to be, sir, your most obedient servant,
Forest Lodge, Maresfield, Uckfield: WILLIAM NOBLE.
January 13th, 1877.

Sir,—Capt. Noble appears to me to have made an unaccountable mistake in his letter (p. 16) on this subject.

I would ask him to place on his table a lighted candle to represent the sun, and a polished globe (a lamp glass will do very well) to represent Venus. If he will then stand so that Venus is in quadrature, he will readily perceive the bright spot half-way between the terminator and the limb, and he then places himself in such a position that Venus shows a crescent, he will still see the bright spot, and will no longer say, at least on this ground, that "it is physically impossible that Mr. Brett can have done so."' *Solvitur ambuldndo.*

Jan. 6, 1877. I am sir, yours faithfully,
W. J. B. RICHARDS.

VENUS'S GLASS AND MIRROR.

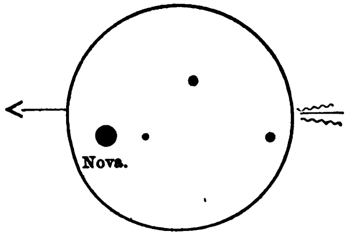
Sir,—One is tempted to ask what is coming to the Royal Astronomical Society when such a paper as Mr. Brett's, on the bright ring seen round Venus during transit, is allowed by the council to be read before a meeting of the fellows. I can only judge by the reports, possibly imperfect, which I have hitherto seen, and it may be that the report you will give in the February number will correct erroneous impressions which have got abroad on this subject. But I find it stated that at a meeting of the Society, which is supposed to consist of the astronomers of Great Britain *par excellence*, Mr. Brett deliberately stated his belief that Venus has a burnished reflecting surface, encased in a glass envelope, and resembles in fact a great thermometer-bulb. All that the spectroscope has taught us, all we have learned of meteoric and cometic systems, and of the laws of cosmical evolution, the laws of optics, and in fine the teachings of every department of modern astronomy, seem of no weight whatever with the excellent artist whose paintings of Scilly scenery delighted us so much a year or so since (*Toujours perdrix, &c.*). Not content with seeing the corona in the full blaze of sunlight, and setting globes as large as the earth bobbing in the atmosphere of Jupiter, he would now set astronomers to look during the transit of Venus in 1882, for an image of the earth reflected in Venus's looking-glass! What next? and next.

Mr. Christie, from whom the Society has a right to expect opinions of weight, seems either to have been poking fun at the meeting, in his comments on Mr. Brett's extravagant notion, or else, in a rare spirit of knight-errantry, to have desired to save a friend from ridicule, which otherwise must have been unanimous. Mr. Christie's well-wishers, of whom I am one of the warmest, have reason to desire that either he may become less Quixotic or his friend less fanciful and better informed.

RICHD. A. PROCTOR.

NEW VARIABLE STAR IN CYGNUS.

The following note as to the position of the new variable star in Cygnus has been sent to us by Mr. Isaac Ward, of Belfast. Its position is—R.A. 21h. 36m. 50·41s. Dec. +42° 16′ 38″·5.



Nova in Cygnus.

As observed by Mr. I. W. Ward, on 30 Dec. 1876, with a 4·3 inch achromatic, power 240.

The new star in Cygnus, discovered by Schmidt at Athens, on 24 Nov., 1876, appeared to be about 6½ magnitude on 30 Dec., and of a reddish cast. There is a 12 mag. companion about 50″ f., and two 9½ magnitude stars form a triangle with the new star.

RADIANT POINTS OF SHOOTING STARS.

Sir,—In December, from the observation of 163 shooting stars, seen in 20½ hours watching (chiefly in the evenings during the first half of the month), I amply confirmed several positions of radiant points, as given in my note (see *Register*, Jan. p. 18), and observed that several of the showers there mentioned were still going on. The position of two of these, as I gave them, require revision, for the additional paths recorded in December indicate the radiant with greater accuracy than the few I had noted in November. The position of the new shower in Sextans I now deduce at R.A. 148°, D. 2 N., and that at τ Leonis as near Crater at R.A. 165, Dec. 6 S. These new radiants give very swift meteors, rather bright, and generally leaving white streaks for two or three secs. in their tracks.

Ashley Down, Bristol:
Jan. 4, 1877.

Your obedient servant,
W. F. DENNING.

TOTAL ECLIPSE OF THE MOON, FEBRUARY 27.

Partially Visible at Greenwich.	G. M. T. h. m.
First contact with the penumbra, Feb. 27	4 33·9
First contact with the shadow	5 29·6
Beginning of the total phase	6 27·3
Middle of the eclipse...	7 15·3
End of total phase	8 3·3
Last contact with the shadow	9 1·0
Last contact with penumbra	9 56.7

Magnitude of the eclipse (moon's diameter = 1), 1 663.

The first contact with the shadow occurs at 124° from the northernmost point of the moon's limb towards the east.

The last contact at 68° toward the west; in each case for *direct* image.

At Greenwich the moon will rise at 5h. 28m.

THE PLANETS FOR FEBRUARY.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	20 6 16	S. 16 47½	9″·9	23 15·3
	9th	20 0 44	S. 18 9	8″·8	22 38·3
	17th	20 22 23	S. 18 56	7″·3	22 28·4
	25th	20 58 22	S. 18 0	6″·3	22 32·8
Venus ...	1st	19 28 28	S. 21 58	11″·0	22 37·5
	9th	20 10 49	S. 20 29	10″·7	22 48·3
	17th	20 52 9	S. 18 21	10″·5	22 58·1
	25th	21 32 18	S. 15 39½	10″·4	23 6·7
Mars ...	1st	16 42 59	S. 21 59½	6″·2	19 52·5
	9th	17 5 23	S. 22 41½	6″·4	19 43·4
	17th	17 27 58	S. 23 11½	6″·8	19 34·5
	25th	17 50 39	S. 23 30½	7″·0	19 25·6
Jupiter ...	16th	17 52 9	S. 23 1½	32″·4	20 2·5
	24th	17 57 29	S 23 2	33″·0	19 36·4
Uranus ...	2nd	9 42 34	N. 14 34	4″·2	12 49·3
	18th	9 39 50	N. 14 48	4″·2	11 43·7
Neptune ...	2nd	2 4 12	N. 10 44½	...	5 12·2
	18th	2 5 11	N. 10 50½	...	4 10·3

Mercury rises about an hour before the sun throughout the month.

Venus may be observed for about an hour before sunrise, at the beginning of the month, the interval gradually decreasing.

Mars rises nearly four hours before the sun on the 1st, the interval slightly decreasing.

Jupiter may be seen in the morning 2h. 40m. before the sun at the beginning of the month, the interval increasing.

ASTRONOMICAL OCCURRENCES FOR FEBRUARY, 1877.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Sirius.
Thur	1		Sidereal Time at Mean Noon, 20h. 47m. 11·01s.			9 50·9
Fri	2		Sun's Meridian Passage 14m. 2·05s. after Mean Noon			9 47·0
Sat	3			3rd Tr. E.	18 56	9 43·1
Sun	4	16 59	☾ Moon's Last Quarter			9 39·1
Mon	5	19 28	Near approach of 4 Scorpii (6)	2nd Sh. I. 3rd Tr. I.	17 29 19 25	9 35·2
Tues	6	17 13	Occultation reappearance of B.A.C 5603 (6½)			9 31·3
Wed	7	2 17	Conjunction of Moon and Mars 5° 28′ N. Conjunction of Venus and Mercury 2° 52′ N.	2nd Oc. R. 1st Sh. I. 1st Tr. I.	17 13 17 28 18 29	9 27·3
Thur	8	0	Conjunction of Moon and Jupiter 5° 28′ N.	1st Oc. R.	17 54	9 23·4
Fri	9					9 19·5
Sat	10	14 18 20	Conjunction of Moon and Mercury 5° 38′ N. Opposition of Uranus and Sun Conjunction of Moon and Venus 2° 59′ N.	3rd Sh. I. 3rd Sh. E.	16 32 18 58	9 15·5
Sun	11					9 11·6
Mon	12	20 58	● New Moon			9 7·7
Tues	13					9 3·7
Wed	14	3	Conjunction of Moon and Saturn 1° 42′ S. Illuminated portion of disc of Venus=0·940 Illuminated portion of disc of Mars=0·908			8 59·8
Thur	15			1st Ec. D.	16 33 55	8 55·9

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Sirius. —
Fri	16		Sidereal Time at Mean Noon 21h. 46m. 19·34s.	1st Tr. E	17 11	8 51·9
Sat	17		Sun's Meridian Passage 14m. 14·74s. after Mean Noon			8 48·0
Sun	18					8 44·1
Mon	19	12 39	Occultation of ε Arietis (4½) Saturn's Ring: Major axis=35″·11 Minor axis=3″·22			8 40·1
Tues	20	16 15 8 39 8 57	☽ Moon's First Quarter Near approach of 19 Tauri (5) Near approach of 20 Tauri (5)			8 36·2
Wed	21			2nd Ec. D. 3rd Oc. R.	17 38 14 17 40	8 32·3
Thur	22			1st Ec. D.	18 27 32	8 28·3
Fri	23			1st Tr. I. 2nd Tr. E. 1st Sh. E.	16 52 17 2 17 58	8 24·4
Sat	24			1st Oc. R.	16 19	8 20·5
Sun	25	13 28 14 26	Occultation of 80 Cancri (6½) Reappearance of ditto			8 16·5
Mon	26	12 46 13 51	Occultation of α Leonis (1½) Reappearance of ditto			8 12·6
Tues	27	7 14	○ Full Moon Eclipse of the Moon visible at Greenwich			8 8·7
Wed	28	15	Conjunction of Saturn and Sun	3rd Ec. R.	16 51 56	8 4·7
MA Thur	R. 1	3	Conjunction of Jupiter and Mars 0° 32′ S. Near approach of χ Virginis (5)			8 0·8

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN FEBRUARY, 1877.

By W. R. Birt, F.R.A.S., F.M.S.

Three portions of the moon's surface have of late been brought into notice, viz., the celebrated crater **Linne**; a portion of the **Crisian Sea**, carefully surveyed in April, 1864, by the Rev. T. W. Webb, and **Fracastorius**, which has occupied our attention for some months past. The *English Mechanic*, January 5, 1877, p. 406, contains a letter from Mr. Knott recording his observations on the evening of December 22, 1876, at 7.30 G. M. T., *et seq.*, from which it appears that the surroundings of the crater differ from those ascertained to exist in the years 1866 to 1868 and following years. At present we shall only solicit the attention of observers to Mr. Knott's letter with the hope that they will carefully scrutinize this most interesting object.

On comparing the drawing of the Mare Crisium by Mr. Webb, especially on the eastern side, in the *Intellectual Observer*, April, 1864, p. 196, with Neison's Map I. some differences were noticed. Webb records and delineates *seven* distinct objects near the eastern border, all of which have been symbolised and entered in the Lunar Catalogue. They consist, *First* of a ridge recorded by Schröter. *Second* of two rings, one only of which was discovered by Schröter, the other was first seen by Birt, August, 1863; the ridge had been seen and described by Schröter many times, yet he failed to detect the ring, a part of the ridge forming its eastern portion. *Third*, of two craters recorded on several occasions by Schröter. *Fourth*, of two craterlets discovered by Webb, which had not been recorded previously. Of these seven objects Neison in Map I. gives *five* only. Part of the ridge; the two rings, both incomplete, and Schröter's two craters; in his map appears a complete ring of which there is no trace on Webb's drawing except the *straight* ridge which forms the western border of Neison's ring. During the spring months many opportunities may occur for ascertaining if Neison's complete ring really exists, it being very improbable that both Webb and Birt should have overlooked it; as in this case the two drawings are so different, drawings of the eastern part of the Mare Crisium will be of great importance for elucidating the question of "change" in the comparatively short period of thirteen years. In the years 1864 and 1865 no less then 49 rings, craters and craterlets were ascertained to exist on the surface of the Mare Crisium, see Report of the British Association for the Advancement of Science, 1865, p. 292. Neison has only 13 of these objects, viz., 12 of the earlier recorded rings and craters and the ring on Schröter's ridge, discovered by Birt but given by Neison as open on the north. Notices of the remaining 36 will be acceptable.

Fracastorius and the southern part of the **Mare Nectaris** still continue to be subjects of observation. In a drawing by Mr. Dennett, dated 1876, November 29th, 10h. 0m., G. M. T., he gives the light streak crossing the crater "Rosse" from S.W. to N.E. as having a companion streak diverging from "Rosse" on the S.E. side of the ordinary streak. Drawings of the light streak on the Mare Nectaris at or about the time of full moon will be much esteemed.

Explanation of Names on the Map of the Moon, I., see Vol. XIV. p. 257, *et seq. Alexander* (B.) the great. The formation is nearly on the same meridian as Julius Cæsar, p. 260. *The Harbinger Mts.* (B.). These mountains are situated to the west of the brilliant spot, Aristarchus, their appearance on the day side of the moon heralds the approach to sunlight of the brightest spot on the moon. The following names have been left out on p. 260:—Jacobi, Janssen, Julius Cæsar. Vol. XV., Piton (B.). A mountain crest in the Island of Teneriffe. Piazzi Smyth, whose crater is in the neighbourhood, speaks of it in his "Astronomer's Experiment," as the "Piton" crest of the oldest of the triple heads of one central peak or cone of eruption. Rambleta. On the moon it is in a line with, but some distance from, the Teneriffe Mountains Chajona, Rambleta, Alta-Vista, etc. It is a little S.W. of Piazzi Smyth, which is between it and Pico, the latter being well known in the nomenclature of the Island of Teneriffe as the "Peak." See map and sections in "An Astronomical Experiment."

Errata.—List for November, 1876, page 271, line 8, for X. read XI.
„ January, 1877, page 24, line 5, for XXX. read * * *

SATELLITES OF URANUS.

The major and minor semi-axes *a* and *b* of the apparent orbits and the angle of position *p*°. of the major axes are the following :—

0h. G.M.T.		Ariel.		Umbriel.		Titania.		Oberon.	
1877.	*p*°.	*a.*	*b.*	*a.*	*b.*	*a.*	*b.*	*a.*	*b.*
Jan.	°	"	"	"	"	"	"	"	"
30.	10·41	15·19	5·77	21·17	8·04	34·72	13·19	46·43	17·64
Feb.									
4.	10·34	15·21	5·83	21·19	8·12	34·75	13·32	46·47	17·81
9.	10·27	15·22	5·88	21·20	8·20	34·77	13·45	46·49	17·98
14.	10·21	15·22	5·94	21·20	8·27	34·77	13·57	46·49	18·14
19.	10·14	15·21	5·99	21·19	8·34	34·76	13·68	46·48	18·29
24.	10·07	15·20	6·03	21·17	8·40	34·73	13·79	46·44	18·44
March									
1.	10·01	15·18	6·08	21·14	8·46	34·68	13·88	46·38	18·57

In these apparent orbits the satellites move so that the angles of position decrease, or the direction of their motion is that of the (ordinary) hands of a watch. For the purpose of knowing whereabout the satellites are to be found, it will, perhaps, be sufficient to learn the approximate times of their being at the ends of the major axis, or at their greatest northern and southern elongations N. and S. In the cases of Titania and Oberon the approximate hours of their being at the ends of the minor axis, or at their superior and inferior conjunctions, "sup." and "inf." are added.

	Ariel. G. M. T.			Umbriel. G. M. T.			Titania. G. M. T.	
1877.	h.			h.			h.	
Feb. 1.	15	S.	Feb. 2.	15	N.	Feb. 13.	3	inf.
2.	22	N.	4.	17	S.	15.	7	S.
4.	4	S.	6.	19	N.	17.	11	sup.
5.	10	N.	8.	20	S.	19.	16	N.
6.	16	S.	10.	22	N.	21.	20	inf.
7.	23	N.	13.	0	S.	24.	0	S.
9.	5	S.	15.	2	N.	26.	4	sup.
10.	11	N.	17.	3	S.	28.	8	N.
11.	18	S.	19.	5	N.	Mar. 2.	13	inf.
13.	0	N.	21.	7	S.			
14.	6	S.	23.	8	N.			
15.	12	N.	25.	10	S.			
16.	18	S.	27.	12	N.			
18.	1	N.	Mar. 1.	14	S.			

					Titania.				Oberon.		
	19.	7	S.					Feb.	1.	13	inf.
	20.	13	N.						4.	22	S.
	21.	19	S.						8.	7	sup.
	23.	2	N.						11.	16	N.
	24.	8	S.	Feb.	2.	6	N.		15.	1	inf.
	25.	14	N.		4.	10	inf.		18.	9	S.
	26.	20	S.		6.	14	S.		21.	18	sup.
	28.	3	N.		8.	18	sup.		25.	3	S.
Mar.	1.	9	S.		10.	23	N.		28.	12	inf.

A. M.

Books received.—" A Summary or Index of Double Stars. Volume I. of Dun Echt Observatory publications. By Lord Lindsay. Dun Echt, Aberdeen. 1876." —The World of Comets. By A. Guillemin, Translated by A. Glaisher. London: Sampson Low & Co. 1877." —"Astronomical Myths, based on Flammarion's 'History of the Heavens.' By John F. Blake. Macmillan & Co. 1877." "On the Visibility of the dark side of Venus. By Professor A. Schafarik, of Prague. (From the Report of the British Association for the Advancement of Science for 1873)."—"Centrifugal Force and Gravitation. The centripetal force and tangential impulse, the law of cyclal-dynamic equilibrial force, &c., &c. By John Harris. Revised edition. Trübner & Co. November, 1876."—"Cours d'Astronomie, Mesure de la Distance du Soleil. Par John Harris. Paris, and Trübner & Co., London."

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Dec., 1876.

Barneby, T.
Bonomi, Dr.
Erck, W.
Lawton, W.

To Jan., 1877.

Barber, Rev. R. W.
Buffham, T. H.
Wilson, C.

To March, 1877.

Elliott, R.
Rivaz, Miss

To April, 1877.

Elger, T. G.

To June, 1877.

Hubbersty, Rev. R. C.
Metcalfe, Rev. W. R.
Ward, I. W.

To Aug., 1877.

Holland, P.

To Nov., 1877.

Harris, J.

To Dec. 1877.

Backhouse, T. W.
Baldelli, Contessa
Barber, J. T.
Box, A. M.
Collingwood, E. J.
Corder, H.
Duncan, J.
Ellis, W. M.
Goode, J. F.
Jefferies, J.
Johnson, R. C.
Jones, Rev. E.
Lamb, Dr.
Lettsom, W. G.
Loddiges, C.
Murgatroyd, J.
Numsen, W. H.
Parnell, J.
Perigal, H.
Perry, Rev. S. J.
Redpath, H. S.
Richards, Rev. W. J. B
Shafferik, Dr.
Slugg, J. T.
Smyth, Prof. Piazzi
Walker, G. J.
Warriner, H.
Whitbread, S. C.
Williams, G.
Wilson, Thomas

TO CORRESPONDENTS.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

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All Letters requiring an answer must enclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or *penny* postage stamps, but the Editor will not be liable for loss in transmission.

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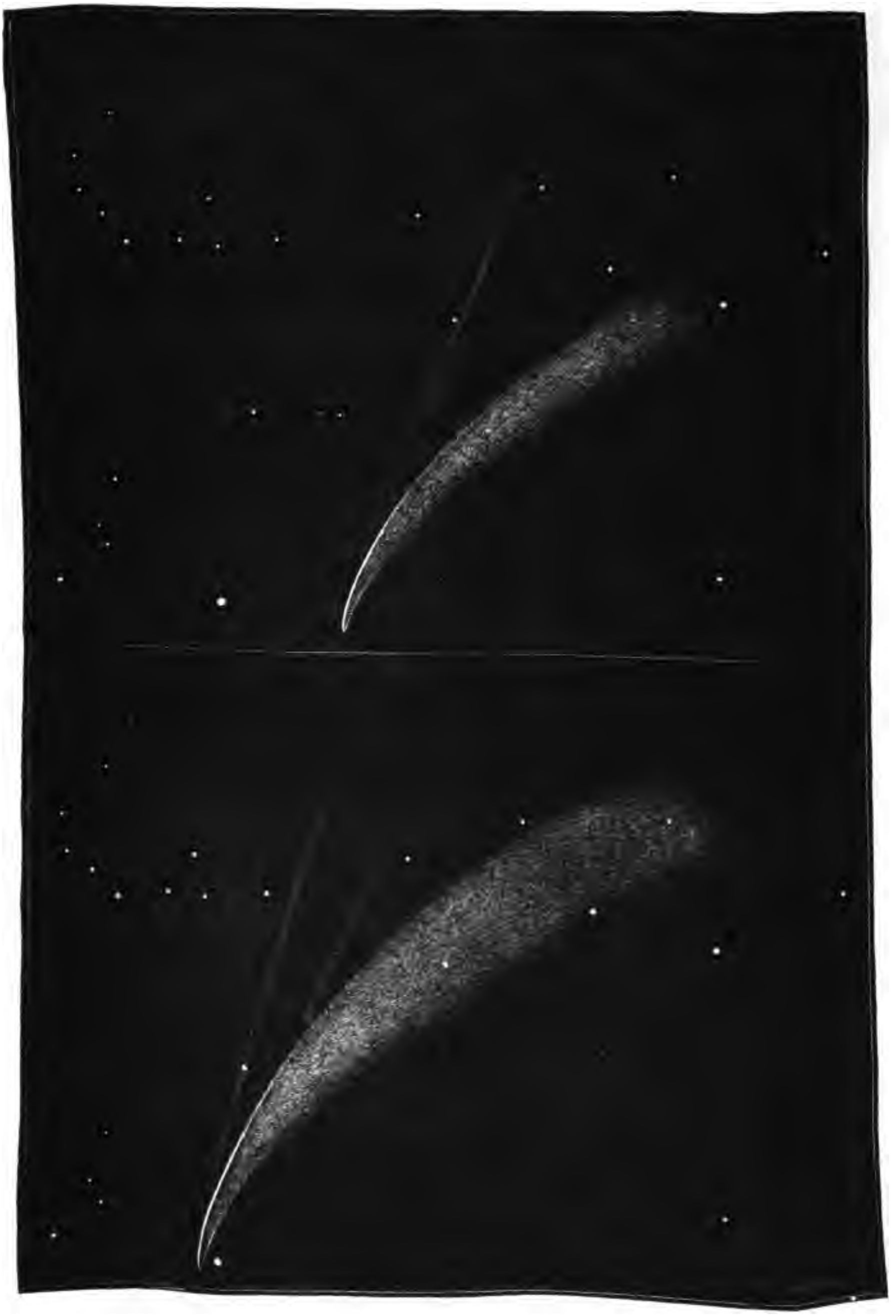


DONATI'S COMET, 1858.

Formation and Development of Cometary Appendages, from drawings by P. G. Bond.

1. Oct. 3, 1858. 2. Oct. 5, 1858.

The Astronomical Register.

No. 171. MARCH. 1877.

ROYAL ASTRONOMICAL SOCIETY.

Session 1876—77.

The General Meeting, February 9th, 1877.

William Huggins, Esq., D.C.L., LL.D., F.R.S., &c., *President*, in the Chair.

Secretaries—Mr. Dunkin and Mr. Ranyard.

The minutes of the preceding General Meeting were read and confirmed.

John Sidney White, Esq., 13, Bolton Gardens, South Kensington, S.W.
A. Mason Worthington, Esq., of Sandiway House, Altrincham.
George Francis Hardy, of Rothbury House, Stroud Green, Finsbury Park, N.,

were duly elected Fellows of the Society.

The annual report of the Society shows that two legacies have been received in the course of the past year; the one a sum of £2,000 Consols, under the will of the late Mr. Carrington, and the other a sum of £200 under the will of the late Mr. T. C. Janson. Twenty-nine new Fellows have been elected in the course of the past year, and after making deductions for deaths, resignations, and expulsions, the number of Fellows has increased by 12 in the course of the year.

Amongst the obituary notices is one of Miss Anne Sheepshanks, a sister of the Rev. R. Sheepshanks, who was for many years a secretary of the Society. Miss Sheepshanks founded during her lifetime, in memory of her brother, the well-known astronomical scholarship which bears his name in connection with Trinity College, Cambridge. She also handsomely endowed the Cambridge Observatory, and at her death bequeathed to the Astronomical Society many valuable books from her brother's library.

The Society's library has also been enriched in the course of the past year by the presentation of several manuscript volumes of sun-spot observations, made by Mr. Carrington, as well as by a valuable series of sun-spot drawings made by the Rev. F. Howlett. The sets of astronomical serials and reports of observatories belonging to the Society are still very imperfect, although much has been done during the past year to fill up the *lacunæ*. The Council report that the number of books taken out of the library is considerably greater than in former years.

In the section devoted to the progress of astronomy during the past year, is a report upon the present state of the reductions which are being made of the observations of the transit of Venus. All the observations with transit instruments for local time and longitude of the different stations are completely reduced. The longitudes of Honolulu and Rodrigues by the observation of the moon in zenith distance are also completely reduced. An idea of the amount of work involved in the complete reduction of the observations will be obtained when we state that these two latter calculations required the use of 3,000,000 figures. It is impossible to form even a moderately accurate estimate of the time which will be required to finish all the work. The reductions which have been made up to the present time have occupied more time than was expected, and two or three years will probably elapse before the work will be completely finished.

In the report upon the progress of solar physics during the past year, it is noticed that, although the sun has been almost free from spots, the height of the chromosphere has not much diminished. Eruptions of magnesium are as active as ever, though hydrogen prominences and metallic eruptions are now very rare. Prof. Tacchini connects the absence of sodium in the chromosphere with the present paucity of sun-spots, and he considers it likely that this element may have a large share in their formation. A discussion has been made by M. Tisserand of the observation of the sun taken by Carrington's method at the Toulouse Observatory in 1874 and 1875, the result being to show that either Carrington's or Spoerer's formulæ will satisfy the observations sufficiently well, though the motions of some of the individual spots are very erratic. Prof. Young has also endeavoured to measure the rate of the sun's rotation by the displacement of the dark lines in the spectra of the solar limb. From the relative displacement of the D lines he finds the equatorial velocity at the east and west limbs is 1·42 miles per second, while direct observation of sun-spots would give 1·25 miles. He is inclined to think that this discordance indicates a physical fact, and that the solar chromosphere, or rather the reversing layer, really sweeps forward over the underlying surface in the same manner in which the equatorial regions outstrip the other parts of the sun's surface. It is, however, worthy of notice that this conclusion is opposed to the results of the Greenwich observations, which are in remarkably close agreement with the received value of the sun's rotation. Prof. Young's observations were made by a fine diffraction grating by Mr. Rutherfurd, in which the spectra of the 6th and 8th orders were observed. The difficulty caused by the overlapping in the spectra of the higher orders was obviated by placing a prism in front of the object-glass of the viewing telescope, with its refracting edge perpendicular to the lines of the grating. The red of one spectrum is thus seen below the yellow of the next, which in turn is seen below the spectrum of the green, and so on. With this diffraction spectroscope Prof. Young has detected the duplicity of the 1474 line. The two components being unequal, the coronal line corresponds to the stronger of the two, while the other is one of the faint lines in the spectrum of iron. The spectroscopic observations of the new star in Cygnus are also referred to. It was discovered by Dr. Schmidt, of Athens, on the 24th Nov. last, when first observed it was of the third magnitude, and of a yellow colour. According to the observations of M. Cornu, its spectrum contains three of the hydrogen lines, C being the brightest—the sodium line D, or the chromosphere line D_3, the magnesium line *b*, and the coronal line 1474. During the past year thirteen minor planets have been discovered, bringing the total number up to 172. The Council report also refers, among other subjects, to M. Trouvelot's physical observations of Saturn, to Montigny's researches on scintillation, and to Mr. Huggins' photographs of the spectra of stars.

After the reading of the report, the President said: It is probably known to all the Fellows of the Society that, as none of the nominees for the medal have received the necessary majority of the votes of the Council which is required by the bye-law, no medal will be awarded this year. Without making any reference to the claims of the gentlemen whose names have been in nomination, I would suggest that it would be well if the award of the medal was regarded as not necessarily annual, but on the contrary that the medal should only be given from time to time, when any nominees come before the Council who have special claims, and I think in this way the prestige of the medal will be surely maintained. (Hear, hear.)

Mr. Barrow: I am charged with the duty of presenting to the Society the report of the Auditors, and in doing so I trust I may be permitted to disabuse the minds of some present of certain misapprehensions as to the position in which the Auditors stand.

It has been a very great grief to us to find that the motives which have actuated us have been misconstrued, and that we are supposed to have been guilty of some intentional discourtesy to the Council. I am charged by my fellow auditors to repudiate in the strongest language I can command any such intention. (Hear, hear.) I will take the liberty of drawing attention to the bye-law which defines the duty of the auditors, perhaps not quite as precisely as could be desired, but at any rate as to the question of time, and the place where the report is to be presented it is sufficiently precise. The bye-law is in these terms: "The account of the Treasurer shall be audited annually, by two or more auditors chosen by the Society at one of the three ordinary meetings immediately preceding the annual general meeting, who shall report at such annual general meeting" (and nowhere else. Now, what are they to report?) "the particulars of the receipts and expenditure of the past year, the balance in hand, and the general state of the funds and property of the Society, and shall also lay on the table a list of the names of those Fellows who may be in arrear for sums due at the last annual general meeting, together with the amount of the same." Now, this bye-law seemed to us, and I think will seem to every person who is in the habit of construing bye-laws, to be sufficiently precise as to the duties of the Auditors. They are to report to the annual general meeting, and nowhere else, those particulars which the bye-law goes on to specify, and I can easily understand why this is. It is to establish the fundamental principle which ought to rule us, and which certainly does rule in every society or collection of persons where there is a large number of outside members, if I may use the expression, supplying the funds, and necessarily a comparatively small number who have the expenditure of them; and that principle is this: that Auditors are appointed who are in no sense officers of the Council, or the governing body, but who are officers for the protection of the great body of the members (hear, hear), and any attempt to interfere with the Auditors, or to control them in any way, by the governing body, whose acts the Auditors have to report upon, is absolutely improper. I go further, and I will say it is illegal. Under these circumstances it appeared to the Auditors that they would better perform their duty—having regard to the fact that it was to a certain extent an innovation, but being prepared to take upon themselves the responsibility of initiating that innovation, in the hope that subsequent Auditors would adopt the same course, we thought it our duty to obey the language of the bye-law, and to reserve our report for this annual general meeting. (Hear, hear.) With this very short preface, I proceed to read the report of the

Auditors, and I hope the Council will not find anything in it which they can find fault with.

The report was then read. In the concluding paragraphs the Auditors acknowledged the assistance they had received from Mr. Wesley, the Assistant-Secretary, and they also referred to improvements in the arrangements of the library, which they asserted were due to Mr. Ranyard.

Mr. Whitbread (Treasurer): As I have been Treasurer for many years, my character is affected by this report. I regret that the time of the annual meeting should be taken up with such trivial complaints against me. (Cries of "No, No.") I beg pardon. I presented my report as Treasurer, and the Auditors scratched out two items. In this they were perfectly wrong. They have produced a report of their own which is not the Treasurer's account. One matter scratched out was my balance. I stated the balance say at £450, that was scratched out and £470 put in, because on the 30th December I had drawn a cheque to pay a person to whom it was due on that day. There had not been time to pass the cheque through the bankers, and the Auditors added the amount to the balance, which was a dishonest statement. The other matter was this: I had entered the handsome legacy of Mr. Carrington as £1880. I put that on one side of the sheet and on the other that it was a sum invested in Consols. The Auditors scratched that out, and you never would have heard a word of it.

Mr. Barrow: You will find it in our account.

Mr. Whitbread: Then you have altered that account.

Mr. Barrow: You have not seen it. You will find it in the paper marked B—the statement of the funded property of the Society.

The President: I think we had better have Mr. Whitbread's statement and then the explanation can be given.

Mr. Barrow: I can explain it in two words satisfactorily.

Mr. Whitbread: I only say my account is the true one and the one produced is not true. That is all I have to say. (Hear, hear and laughter.)

Mr. Barrow: In the statement of account it was absolutely necessary to show that your balance corresponded with the balance at the bankers.

Mr. Whitbread: Not a bit.

Mr. Barrow: The balance at the bankers on the 31st Dec., 1876, is the balance shown by our figures. There was an outstanding cheque which when the bankers had paid it would have reduced the balance by £15. We have chosen to put down in our figures the balance as it actually did exist in accordance with the bankers' book on the 31st December, 1876. We have on the other side of the account put in the cheque as a debit for £15, because inasmuch as the cheque was not then paid it still remained a debit to the person in whose hands the cheque was. And the reason is this: Supposing at a future time it should be necessary to check for some years the accounts of this Society, what would be said if you took up a balance-sheet and found that the balance at the bankers represented by a certain sum, and then found that the bankers' book gave another sum as the balance? (Hear, hear.) The whole question is a mere matter of form, and not of substance, but we maintain that our report is right. With regard to the Carrington legacy, we are again, I say, absolutely correct. The cash account that this item appears in should be an account of the sums that have actually reached the Treasurer's hands, and been paid out by the Treasurer, and it is perfectly incorrect to say that any sum of £1880 has been paid into the Treasurer's hands and paid out on account of the Carrington legacy. What has taken place is this: The trustees or executors of the late Mr. Carrington have transferred, not to the name of the Treasurer (the Treasurer has nothing to do with it), but to the Royal Astronomical Society, as a corporate body, the sum of £2000 Consols. You will find that in our cash account that sum does not appear. It is not cash, and never has been, but if you will look at the paper marked B, you will see our report states that the funded property of the Society has been increased by the sum of £2000, the bequest of Mr. Carrington, and we as auditors maintain that that was the proper place to report it, and not to treat it as cash, which it will not be till it is sold out.

Mr. Whitbread: You maintain that the Treasurer has nothing to do with that £1,880?

Mr. Barrow: Yes.

Mr. Whitbread: I was sent for by the executors. My name was mentioned. It was paid to me personally upon the receipt which I gave, and I brought the sum home.

Mr. Barrow: It is reported to us there is no cash, but a transfer of stock.

Mr. Whitbread: You said I had nothing to do with it. I received it. It was I who was mentioned as entitled to receive it. I gave a proper receipt for it, and I invested it in Consols.

Mr. Barrow: I said it was not cash.

Mr. Whitbread: It was cash to me.

Professor Adams: Is not this "a storm in a teapot"? (hear, hear.) There is really no practical difference between the auditors and the Treasurer. I really do not think it worth wasting any more time about.

The Rev. T. J. Main: I think if the Treasurer's account is correct we ought to support him. He is quite right in crediting the bank with £15, inasmuch as it was paid.

Mr. Barrow: It was not paid.

The President: I think it is purely a matter of form. We have heard both sides. On the one hand the Treasurer's report is signed by him, and has been inserted in the report of the Council; on the other hand the auditors are quite right in reporting in what form they please. We have not to discuss the form in which they choose to hand in their statements. The members will see there is no difference whatever between the two accounts. First of all the Treasurer has brought into his account the Carrington bequest as money received on the one hand, and money invested on the other. He has given a receipt, certainly; but the Auditors conceive from their point of view that it should not be entered in his financial statement as cash, and they have placed a special report upon the receipt of this money in a separate schedule. The other point is this small matter of the cheque. There had not been time for the cheque to be paid by the bankers, and therefore in the auditors' report it is given as "cheque outstanding," and in the treasurer's statement the balance is reduced so much. I think now that all the Fellows will prefer that the discussion should cease. (Hear, hear).

Admiral Ommanney: From the applause which you have given to the reading of the Report, I am quite sure that it has met with your satisfaction. Therefore I need not say much in support of the motion I am about to move. It must be gratifying to all the Fellows to find that the influence of the Society is extending itself all over the world. One particular part of the Report I am sure you must have been glad to hear, viz., that which referred to the efforts of the Council to improve the library, the condition of which is much more satisfactory now than it has ever been before. I beg to move "That the report of the Council now read be received and adopted, and that it be printed and circulated in the usual manner."

Mr. Phillips: I will second that motion, but I will not detain you with any remarks.

Commander Verney: In the report it is stated that several numbers of serials are missing from the library. I would suggest that a catalogue of the missing numbers should be printed, and circulated among the Fellows, so that we may have the opportunity of picking them up and sending them to the librarian.

The President: A very important suggestion.

The motion for the adoption of the report was then carried.

Sir E. Beckett: I move that the report of the auditors be printed. I do so because I find it was not printed last year. I do not know why. I am not in the secret; but all I can say from my experience of other societies is that as a matter of course the report of the auditors should be printed. I have had professionally to do with a good many of the consequences of what I can only call slovenliness in dealing with auditors' reports, and I have had to do with affairs where millions of money have been wasted, because the auditors have been told by the governing body that it was their duty to accept whatever was put before them. They accepted the snubbing of the directors and so the officers of the society gradually advanced from small irregularities to large frauds (laughter). Far be it from me to say there has been any irregularity here, except of the most technical kind, or that the other thing is at all likely to take place here (renewed laughter), but I know that in consequence of such proceedings as I refer to, a very stringent clause has been introduced into a great many public acts, making the auditors absolute masters of the situation. It does not follow that we shall agree with the auditors if we print their report, but we want it to be known, not only to the hundred or so persons present, but to the other five hundred Fellows of the Society who are not here to-day. I find that last year the report of the auditors was not printed on account of an express order of the Council.

Mr. De La Rue said that the Council last year decided that the auditors' report should not be printed as a part of the Council's report, because it contained eulogiums upon officers quite beyond the duties of the auditors, and for the same reason he should object to the latter part of the present report being printed.

Commander Verney seconded Sir E. Beckett's motion.

Mr. De La Rue: I shall move as an amendment that the last two paragraphs in the auditors' report, referring to Mr. Wesley and Mr. Ranyard, be not printed as part of the report of the auditors. I do so on the simple ground that it is no part of the functions of the auditors to pass eulogiums upon any portion of the work done by the officers. In other respects I would gladly see the report printed.

The Rev. Robert Main seconded the amendment.

Mr. G. F. Chambers said the auditors, in their last paragraphs, have referred to the condition of the library. I have, during the last two years, had occasion to make great use of the library, and I desire to say that I think the eulogiums passed by the auditors on Mr. Wesley and Mr. Ranyard are well deserved (hear, hear); and I will go farther, and say it would be unjust to omit the eulogiums of the auditors from their report. I concur with Sir E. Beckett that the auditors' report ought to be printed. The condition of the library has for many years been a disgrace; there is no means of knowing what books are in it; and I am glad to welcome the changes introduced by Mr. Ranyard.

Mr. Bidder: I think there must be an immense amount of electricity in the air (laughter), and that Mr. De La Rue must be unusually sensitive to propose such an amendment. It seems to me hardly worth while to discuss whether the auditors have exceeded their duty when the only question is as to acknowledging the zeal of our officers in the discharge of their duties.

Mr. De La Rue said: My objection is that this will form a precedent, and might become extremely inconvenient. If the auditors would strike out that part of the report, and then propose a vote of thanks to Mr. Wesley and Mr. Ranyard, I would join heartily in the vote. (Hear, hear.)

The Astronomer-Royal said Mr. De La Rue had anticipated him in regard to this point; he (the Astronomer-Royal) thought it of the highest importance that the auditors should act strictly in accordance with the rules laid down.

Sir E. Beckett said: If the auditors choose to strike out the last two paragraphs of their report and move a vote of thanks instead to the officers named, I shall have no objection to that course being taken. But we have no right to do it for them. (Hear, hear).

Mr. Esdaile said: I think, sir, that the auditors are quite in order in recognizing the facilities which have been afforded them in doing their work. In former years, owing to a want of proper arrangement in the library, the auditors have found it impossible to make any report upon the subject.

Mr. Brewin said: The reference in our report to Mr. Wesley and Mr. Ranyard has regard only to the assistance given us as auditors, but if it is the wish of the meeting that the auditors should withdraw that part of the report and substitute a vote of thanks, I as one of them shall be very happy to do so.

General Boileau thought the auditors had done wrong in introducing any remarks of approbation on any of the officers; and that a vote of thanks on the part of the Society was the more appropriate way of proceeding. He hoped the mover of the motion would accept the amendment.

The President said that he thought that the acceptance of the amendment would meet the general feeling of the meeting, it being understood that the auditors would move a vote of thanks afterwards.

The amendment of Mr. De La Rue was then carried unanimously, and Mr. Brewin proposed a vote of thanks to Mr. Wesley for the assistance he had rendered to the auditors, and to Mr. Ranyard for the care he had bestowed on the library and the Society's publications.

Mr Barrow seconded the motion, and it was agreed to unanimously.

The President: The next business is the election of Council and Officers for the coming year, and I will appoint Capt. Toynbee, Dr. Mann, and Mr. Gill scrutineers.

Mr. Bidder: I wish to ask two questions affecting the ballot. If a vote is given for a candidate for any special office, as that of treasurer, or secretary, or vice-president, and he is not elected, does the vote count to him for election as an ordinary member of the council? (Mr. De La Rue: "No.") Then I will pass on to the other matter. I observe in the house list that we have lost both of our secretaries. Neither Mr. Dunkin's nor Mr. Ranyard's name appears in the list as candidates for the secretaryship. As regards Mr. Dunkin, we know the reason. He is going to a higher and better place to rest (laughter)—to the well-earned rest of a vice-president's chair (hear, hear), but that is all the more reason why we should not lose our other secretary, who, everybody feels, has worked most efficiently. One can hardly help assuming, looking at this list, that the Council has not succeeded in persuading Mr. Ranyard to stand again; but I think there will be but one opinion amongst us, that we cannot afford to lose Mr. Ranyard (hear, hear), and therefore I will ask Mr. Ranyard, if he is elected, whether he will consent to act again as Secretary?

Mr. Ranyard: If you do me the honour to elect me, I shall be happy to serve. (Hear, hear).

Mr. Bidder: Now we hear that, I hope we shall all be of one mind, without any slight to Capt. Abney or Mr. Glaisher, and substitute Mr. Ranyard for one of them.

Mr. Mummery: Might we be informed why Mr. Ranyard has ceased to be one of our secretaries? He has discharged the duties so efficiently, and rendered us such valuable aid, in connection with the library, that I feel that the loss of his services would be a calamity to the Society, therefore it is to me a great satisfaction to hear that Mr. Ranyard, if elected, will serve us again.

Mr. Bidder: I suppose Mr. Glaisher will not think it a slight to him if we put Mr. Ranyard in his place.

Professor Adams: I suppose any Fellow can vote for whom he pleases.

Mr. Perry: I should object to any person being excluded by name.

Mr. Bidder: Mr. Glaisher is a personal friend of my own. Pray do not suppose I mentioned his name because I supposed he was not a fit person.

Mr Dunkin: Mr. Bidder has kindly told you that I am to be sent to the higher regions; whether for the benefit of the Society or not I really hardly know; but I should like to make a personal remark about the secretaries, particularly about myself. I have received letters during the last week from several Fellows, asking me whether it is true that I am being shelved and put out of the secretaryship to please a certain party, and that Mr. Ranyard and myself were both shelved for the same purpose. I may explain, speaking for myself, that my retirement from the secretaryship has nothing whatever to do with any party feeling, but arises from private reasons. When I received those letters, telling me that letters had been written by members of the Council, saying that I was retiring because I was obliged to retire, I thought it was very unfair to make such representations for electioneering purposes, when everyone knows that my retirement is a source of great regret to myself, for I have enjoyed my tenure of office, and I think every member, from the President on my right down to the junior member of the Council would have been anxious to retain me. (Hear, hear.) With these few remarks, I hope that in choosing your secretaries you will choose the best men, and not be carried away by party feeling.

The ballot was then taken, the fellows coming up to the platform seat by seat to vote. The papers having been all collected and the names of voters taken down, the scrutineers retired with the lists to the council-room, and after the lapse of two hours and a half, returned, and announced that the following gentlemen had been elected:

President.

William Huggins, Esq., D.C.L., LL.D., &c.

Vice-Presidents.

J. C. Adams, Esq., M.A., F R.S.
Sir G. B. Airy, K C.B., LL.D., M.A., F.R.S., Astronomer-Royal.
Prof. Arthur Cayley, M.A., F.R.S.
Edwin Dunkin, Esq., F.R.S.

Treasurer.

Samuel Charles Whitbread, Esq., F.R.S.

Secretaries.

A. Cowper Ranyard Esq., M.A.
J. W. L. Glaisher, Esq., M.A., F.R.S.

Foreign Secretary.

Lord Lindsay, M.P.

Council.

John Brett, Esq.
W. H. M. Christie, Esq., M.A.
Warren De La Rue, Esq., M.A., D.C.L., F.R.S.
J. R. Hind, Esq., F.R.S.
E. B. Knobel, Esq.
George Knott Esq.
William Lassell, Esq., LL.D., F.R.S.
E. Neison, Esq.
Captain William Noble.
Rev. S. J. Perry, F.R.S.
Earl of Rosse, F.R.S.
Captain G. L. Tupman, R.M.A.

Mr. C. V. Walker proposed a vote of thanks to the vice-presidents, secretaries, and general members of the Council who now retire, for their efforts in promoting the objects of the Society.

Mr. H. Perigal seconded the proposition, which was carried.

Professor Adams: Although the gentleman I am about to name is included in the resolution just passed, I think we ought on the present occasion to take especial notice of the merits of the Secretary who retires—Mr. Dunkin (hear, hear), and the services which he has rendered to the Society for so many years. The unsparing efforts he has given to the interests of the Society I am sure are appreciated by all present. I need not say we all regret the cause which has led to his retirement. As he said, it was not on account of any wish of his own, or the Society, but really he was compelled by the state of his health. With comparative rest I hope his health will be re-established. I beg to propose a special vote of thanks to him for his unwearied exertions for so long a time in the interests of the Society.

Mr. De La Rue: I shall be proud of the privilege of seconding the proposal of Professor Adams. All Mr. Dunkin's colleagues know how indefatigable he has been in his devotion to the interests of this Society. At the time of our removal from Somerset House the duties of Mr. Dunkin were exceedingly onerous; and I am afraid that the work on that occasion hastened the death of our late worthy Assistant-Secretary, and threw a great deal of work on Mr. Dunkin: besides the work he does here so thoroughly there is other work which devolves on him that everyone is not aware of. I mean the preparation of the admirable report of the Council, in which the summary of the progress of astronomy, with the exception of that portion relating to shooting stars, is entirely from the hand of Mr. Dunkin. (Hear hear.)

The President: I most cordially agree in the resolution, and will ask you to pass it by acclamation. (Hear, hear.)

Mr. Dunkin: I thank you most cordially for the very kind manner in which you have passed this resolution. Whoever undertakes the duties of the office of Secretary, it is his duty to do the best he can for the Society. On the present occasion you will allow me to say that I really do feel this vote is an ample repayment for all the trouble and anxiety I have had during several years. In retiring from the Secretaryship it is really because I have found that my official duties, which are onerous, in combination with the duties required by this Society, especially at this period of the year, are too much for my health; in fact, last year I broke down. Through the kindness of the Council, they appointed Mr. Glaisher to help me, but I am glad to say that I have been able to attend to the duties at these meetings without exception, though I have frequently sat at this table in great pain, but at the same time I have gone through them. This is the seventh annual meeting that I have sat in the Secretary's chair, and I have never absented myself once either from the Council meeting or this chair at the meetings. After serving the Society so long, I think I may very well retire, and in doing so I am happy to find that my successor is such a distinguished mathematician, and one who I am sure will do honour to the Society as well as honour to himself; it is a great pleasure to me to find that Mr. Glaisher is elected. There has been a strong feeling to prevent his election, but I am rejoiced from the bottom of my heart that he succeeds me as Secretary.

The meeting then adjourned.

REVIEWS.

The Sun: Ruler, Fire, Light, and Life of the Planetary System. By Richard A. Proctor. With nine lithographic plates (seven coloured) and 100 drawings on wood. Third edition. London: Longmans, Green & Co. 1876.

(Continued from page 43.)

The heat, light, and chemical activity of the sun are treated of in Chapter VIII. Observing that at present all our motive force is obtained from *stored* sun-force, and that we are still utilising what Professor Tyndall calls the sun of the carboniferous epoch, Mr. Proctor says, "Our descendants will have to employ the sun of their own epoch. The heat of the solar rays—mayhap also their light and their actinic energy—must one day be applied to work our machinery.' And he gives some striking instances of the enormous amount of energy which may thus become available. The source (or sources) of the solar energy itself is still an unsettled question. "Whether we conceive, with Mayer and Thomson, that the sun's heat is maintained by the incessant downfall of cosmical bodies, gathered out of space by the sun's mighty attractive energies, or whether we follow Helmholtz, in supposing that the gradual contraction of the solar orb is the mainspring of the solar energies, or whether we believe with Secchi, that the dissociation of compound bodies in the sun's substance is a fund of force to be gradually exhausted only as the dissociated elements unite in chemical combinations; or, lastly, whether we prefer the idea thrown out by Sir John Herschel, that mayhap the vital energies of monstrous creatures—the willow-leaves of Nasmyth—are the true source of the great luminary's might, we have not overpassed, by a step, the amazing field of conjecture appertaining to our subject. Startling as these theories appear, they are not a whit more startling than the known facts which they are intended to interpret." The author is inclined to believe that the theories of Helmholtz and Thomson combined may account for a very large proportion of the sun's activity. Uncounted millions of minute cosmical bodies are streaming in upon the sun, and Helmholtz has shown "that the shrinking of the sun's diameter by one 10,000th part of its present length would generate an amount of heat competent to cover the solar emission for 2,000 years."

Chapter IX. "The sun among his peers," is the last of this able monograph. The figure and dimensions of the sidereal system, its structure and complexities, and the motions taking place within its limits, a noble problem of surpassing interest, as Mr. Proctor truly terms it, is one the solution of which is scarcely more than begun. We think the author has laid a good and sound foundation, and that his views are destined to be generally adopted and followed up. He gives us a drawing of the Milky Way regarded as a spiral made up of small stars, with which large suns are associated; and concerning the sun's proper motion he says, "He is passing away from the neighbourhood of Canis Major, Columba and Lepus, (not to define too precisely the as yet scarcely determined path along which he travels) and he is urging his way with inconceivable velocity towards the region between Hercules and Lyra. Of the true habitudes of those regions of space through which he is bearing, and has lately borne, his family of planets we know little. But as we look back along the extended tract he has pursued, and see the richness of those regions he has left, and as we look onwards and trace his course in imagination towards the borders of that rich region whose glories gather into their chiefest splendour in Cygnus, the conception is suggested that he is now urging his way through a relatively barren region, that he has left and will again visit more glorious star-depths than those through which he now pursues his course." The earth's motion through space, in "a skew spiral or helicoidal path," is well illustrated by two diagrams. And then comes the enquiry, Has the sun any other companion suns on his voyage? The difficulty of answering this is explained; but the existence of such companions is pronounced not wholly improbable, and several pictures of groups of stars are given, amongst which the five principal stars of Ursa Major, which it can scarcely be doubted are travelling together through space. These are β, γ, δ, ε, ζ, whilst α is seen to be moving in the opposite direction. To each star is attached a small arrow, indicating the direction of its motion, and the amount of such motion in 36,000 years. We see here decided signs of star-drift. Another evidence of association apart from the observed proper motions, is that Secchi has noted these stars as having very similar spectra—in other words, as resembling each other very closely in structure and condition. "In this circumstance we have a peculiarity which may some day enable us to select with some confidence those stars which are our sun's special companions, which voyage along with him through the sidereal system, and share with him perchance in a reign over some special domain of space."

There is an island celebrated from remote antiquity, the special part of the earth allotted to Helios (the Sun-god). whose eye discerned it before it had yet risen out of the waves of the Ægæan (Pindar, Ol. VII.); of great beauty and fertility, with a delicious climate, proverbially sunny; for the sun is said to shine there every day in the year; once the home of distinguished schools of Greek art and literature; whose capital was famous for its architectural beauty and the large number of statues (3.000) with which it was adorned. There, in very ancient times dwelled the Heliadæ, or sons of Helios, famed, it is said, for their progress in astronomy and navigation. That island is Rhodes (the island of roses). Nowhere else was the sun held in such honour; for to him was raised the bronze statue 70 cubits high, commonly known as the Colossus of Rhodes, reputed one of the seven wonders of the world. We would like to breathe an air fragrant like that of Rhodes from its many orange and citron groves and aromatic herbs; still more would we like to have such skies as there for the use of our telescopes and spectroscopes. But we may rejoice that, in a manner, we moderns too have our Heliadæ,—the skilful, persevering, and cautious students of the sun, both departed and living, whose labours have begun to lift a little the veil that covers the solar mysteries. The names of many are found in the work we have been reviewing; and to its author belongs the merit of bringing together from all quarters the most important facts and theories; of commenting on them with a judgment discriminating and sound; of originality in his own views and hypotheses; and, to say no more, of presenting us with a volume, which, moderate in compass, teems with condensed and perspicuous thought; and those who will not only read but study it, will be in possession of all that theoretic and practical science, aided by refined optical skill, have thus far been able to teach about the Ruler, Fire, Light, and Life of the System in which our lot is cast.

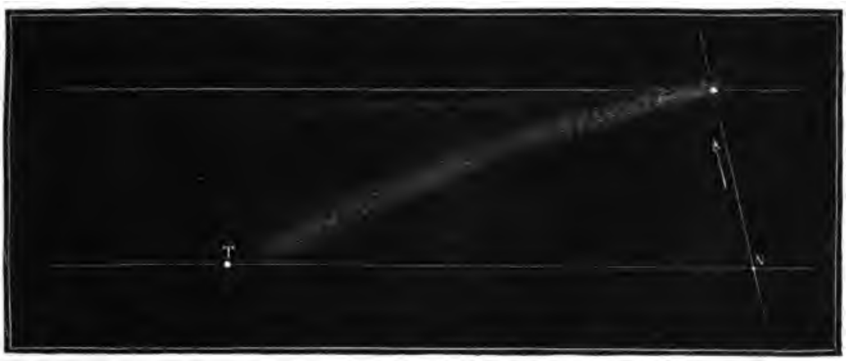
Perhaps it is worth while to note the following errata, &c., though mostly trivial:—

Page 122, line 4 from bottom—*Justice* (?) *justness.*
132, 133, *Fig.* 24 - *Fig.* 25.
157, *Gassenius* - *Gassendi.*
209, line 3 from bottom, *large—larger.*
„ line 2 „ *then—than.*
210, bottom. We believe Hannay's Almanac has been some time discontinued.
245, line 11 from bottom, comma after *spectra* to be removed.
255, line 13 from bottom, *and to the moon—and not to the moon.*
292, line 8, *Fig.* 72—*Fig.* 71.
298, line 3 from bottom, lines *B*—lines *b*.
319, bottom, *larger—longer.*
326, *recognized* - *recognised* (as elsewhere in the book); so p. 335.
327, line 8 from bottom (?) "History of Physical Astronomy."
333, note.—A word (or words) seems wanting.
351, note and p. 352, line 10, *Wenlock* or *Winlock* (?)
389, line 5 from bottom, *from—form* (?)
411, *Seabroke* (?) *Seabrooke.*
429, line 12, *theorize—theorise.*

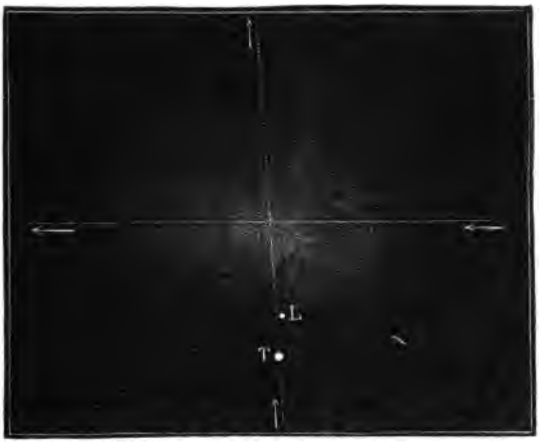
The World of Comets. By Amédée Guillemin, author of "*The Heavens*"; translated and edited by James Glaisher, F.R S. London : Sampson Low and Co., 1877. The following is from the Editor's preface.

"I have great pleasure in introducing to English readers M. Guillemin's valuable and interesting work on comets. * * There is no work that at all occupies the ground covered by that of M. Guillemin : and as the subject is one which, always of high interest, has in the last few years acquired great importance in consequence of Schiaparelli's discovery of a connection between comets and shooting-stars, I was anxious that it should appear in our language. * * Whenever I have thought that additional explanation was desirable, or that the researches of the two years that have elapsed since the publication of the original work threw further light upon the subject, I have added a note of my own. * * In two cases I have ventured to make more lengthy additions of my own—these relate to Coggia's comet, which had only just left us when M. Guillemin's work was published, and the connection of comets and shooting-stars." Coggia's was the first bright comet to which the spectroscope has been applied. Observers will welcome another. Mr. Glaisher says in the last lines of the book (p. 548). "The reader will notice the extraordinary dearth of comets in the last two years. No other comet except Encke's was seen in 1875, and none have been seen during the present year (1876). This complete absence of comets, following years so rich in comets as were 1873 and 1874, is very remarkable."

A work like this was much needed, and the author's lucid, judicious, and very full treatment of the subject has been ably presented to the English reader by Mr. Glaisher, whose additions materially increase the value of the original. The printing, binding, and illustrations are excellent. Of the last mentioned, there are five plates, including two beautiful drawings by Dr. De La Rue of the great comet of 1861 ; five whole-page woodcuts, and seventy-five woodcuts in the text. (One plate, referred to on p 249, we have not found). By the kindness of the publishers we are enabled to give our readers three specimens of these excellent woodcuts. There are two tables ; one of the periodical comets, and the other of comets to the number of 310, whose elements have been calculated with some approach to accuracy. This in all respects - both inside and out - attractive volume we think will become as much appreciated in England as M. Guillemin's former work on "The Heavens." On page 172, the author says, "In our second volume, which will form a continuation of the present work, and which will be devoted to the subject of *Shooting Stars*, we shall give new proofs with regard to the origin of comets. It will be there seen how they arrive from all points of space in their peregrinations from world to world, and wheel around the sun like moths about the flame of a candle, some to be there consumed, and feed the incandescent ruler of our system, others to be dispersed in long trains and shed the dust of their atoms in the interplanetary spaces. The shooting stars, which vary the sublime, but never changing, spectacle of the starry nights of the earth, are but fragments of dispersed comets. Who knows but that the incessant rencontres of the planets with those cosmical atoms may be a means of increasing the planetary masses ? Who knows but that comets play an important part in the formation and evolution of planetary systems? This point, insignificant as it may appear, in contrast with our enormous planet on the one side and our brief existence on the other, may in the course of time exercise a considerable influence upon the earth's mass. For its operation this influence has time—millions of years—and the



Passage of the Earth through tail of Comet of 1861, on June 30.



Positions occupied by the Earth and the Moon in the interior of the second tail of the Comet of 1861.

[*Reprinted from "The World of Comets."*]

nebulous matter "scattered," as Laplace has said, "with such profusion throughout the universe."

Here and there we have noticed a misprint; *e.g.* p. 459, at the end.

Astronomical Myths, based on Flammarion's "History of the Heavens." By John F. Blake. London: Macmillan and Co. 1877.

This is a popular and entertaining work, with a number of illustrations, many of them interesting, some curious, and others merely fanciful. The fifteen chapters are as follows:—The first beginning of Astronomy. Astronomy of the Celts. Origin of the Constellations. The Zodiac. The Pleiades. The nature and structure of the heavens according to the ancients. The celestial harmony. Astronomical systems. The terrestrial world of the ancients,—Cosmography and Geography. Cosmography and Geography of the Church. Legendary worlds of the middle ages. Eclipses and Comets. The greatness and the fall of astrology. Time and the Calendar. The end of the world. It is a pity that so many references to other works, many of which are difficult of access, have not been more carefully made. As it is, inaccuracies are too common, and misspellings of proper names abound. Here are some of the first mentioned:—

Page 94. Visakha, the 16th Indian lunar mansion, corresponds to four stars in Libra, not to the Northern Crown.

Page 117. We read, "in India the year commenced in the month they called Cartiguey, which means the Pleiades." The proper name of this month is *Karttika*, corresponding to parts of October and November. The Pleiades are *Krittika*, supposed to have been the nurses of Karttikêya, the war god. Krittika, now the third, was formerly the first *nakshatra*, or lunar station, and the solar year seems to have commenced when the sun arrived there.

Page 120. "On the 17th day of that month (November) is celebrated the Hindoo Durga, a festival of the dead, &c." This is not very clear. Durga, wife of Shiva, is one of the great Indian goddesses—emblem of the passive energy of nature. At present, in the month Aswin (Sept.—Oct.) during nine days in the moonlit half of it, are ceremonies connected with Durga. The great festival of the dead is held in the last half of *Bhadrapad* (Aug.—Sept.), or new moon of Aswin, which is sacred to the *Pitris*, or progenitors, to whom offerings are made. Possibly, however, the dates of the festivals may have undergone changes to correspond with those which there is reason to believe were made in their mode of commencing the year. It may be observed, too, that Durga has also a festival in Karttika; and that the *sraddhas*, or ceremonies connected with the souls of the departed are prescribed also in other parts of the year.

Page 160. Light from one of the nearest of the fixed stars is said to take no less than seventy-two years to reach us.

Page 236. It is said, "The first astronomical observatory that was constructed was that of Paris." The first national observatory was that of Copenhagen, begun in 1637, and completed in 1656; Longomontanus, pupil of Tycho, being the first Director; too considerable a person to be designated, as on page 214, as "one Longomontanus." Earlier still was Tycho's observatory, Uraniburg; 1576.

Page 245. The vessel of gold which carried the sun is not mentioned in Homer, but in later poets.

Page 250. Speaking of the Hyperboreans we read, "Herodotus regrets that he has not been able to discover the least trace of them; he took the trouble to ask for information about them from their neighbours, the *Arimaspes*, a very clear-sighted race, though having but a single eye;

but they could not inform him where the Hyperboreans dwelt." Herodotus was never near the Arimaspi; and he says he could not bring himself to believe in a race which had but one eye, as the report went about them. Misspellings, as we have said, are numerous; but we can only advert to a few. On *page* 236 we were puzzled what to make of the word *medpiama,* till we concluded it was intended for *madhyama,* a middle or inland country; as *madhyamaloka* means the middle world, as being between heaven and the infernal regions. *Page* 304. There is no such cave as that of "Zenarus" near Lacedæmon. Tænarum is probably meant. On *page* 343 mention is made of the historian Sazoncenas, a portentous name we had never met with. On reflection we perceived the word must be Sozomenus (Sozomen), the well-known historian.

Mr. Blake has done well to introduce Mr. Haliburton's interesting researches on the ancient year of many nations, as regulated by the Pleiades; but we find it difficult to comprehend the following, p. 117. "Among the ancient Egyptians we find the same connection between *Athar-aye*, the name of the Pleiades, with the Chaldeans and Hebrews, and *Athor* in the Egyptian name of November. The Arabs also call the constellation Atauria." Is it meant that the Chaldeans and Hebrews called the Pleiades *Athar-aye?* Or are we to understand that it is the Egyptian name for them? We conclude the latter is intended, since further on, p. 134, speaking of the Egyptians, it is said that their name for these stars was the Atauria. As to the Arabs, the name is Al-thurrayya, pronounced Ath-thurayya, (*al* being the article), and has no relation to the Egyptian Athor, the name of the month November, and of the Egyptian Venus, with whom the Pleiades seem to have been connected. On the Zodiac of Denderah there is a figure beneath the Bull, with seven stars in front of her, which has been taken for Athor and the Pleiades. Macrobius also says the sign of Taurus was assigned to Venus. We know that cows in Egypt were sacred to Athor; and the corresponding Phœnician goddess, Ashtoreth or Astarte, is represented with the head and horns of a heifer. On the same page last referred to we are told the Egyptians "took the Pleiades to indicate the Bull, and called this animal after the Atauria. From thence we get the Latin Taurus, and the German Thier." We need not draw upon fanciful etymologies to show that the year of the Pleiades was known in Egypt; for the Great Pyramid alone seems to be a conclusive witness to this: and we must demur to the above derivation of Taurus. If the Egyptians had such a word, as here stated, for the Pleiades (and we cannot at present verify this), it probably meant the stars of Athor. The usual, and we doubt not the correct, derivation of Taurus is from the Hebrew Shor, ox (irrespective of sex), which by the common interchange of *s* with *th* and *t*, becomes in Arabic Thaur, bull: in Chaldee, Tor; in old Persian Tora, and (according to Plutarch) in Phœnician, Thor.

Page 411. We read, "It would seem most probable, then, that we must look to the Accadians as the originators of our modern week, from whom the Hebrews may have—and, if so, at a very early period—borrowed the idea." The Hebrews, we may be assured, borrowed the week from no nation; and the Accadians probably had it from primitive tradition.

Though an acceptable contribution to our astronomical literature, for which many will feel obliged to the author and his English editor, this work needs to be overhauled and freed from such negligences as we have adverted to. We have suspicions of others, but have not the means of verifying them. A list of works used, and referred to, would have been a useful appendix.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

MR. CHRISTIE'S CIRCULAR.

Sir,—Can any one interpret for us Mr. Christie's circular to the Astronomical Society. It appears he finds that his "character" wants "vindication," because he might be suspected of wittingly opposing Mr. Glaisher for the secretaryship. Now, the Council were divided on this occasion. It is said that four names were balloted for, Mr. Christie's being one by his own consent. If it was wrong (which I do not for a moment think) under these circumstances, for a member of Council, who had had the support of a section of Council, to consent to oppose for the secretaryship one who was as yet only a member of Council, how much more wrong must it have been of Mr. Glaisher, when not as yet on the Council, to consent to oppose my *re*-election in 1873, when there was no division in the Council. But it may be argued I was on Strange's black list. "Events had occurred during the preceding year." Col. Strange had left the Council in October, 1873, because my name was selected for the medal two months afterwards. So he stated in March, 1874, and as truthfully that I had had anything to do with my nomination by a gentleman with whom I had not till then exchanged ten words verbally or by letter. But Mr. Christie could hardly urge that as a distinction between Mr. Glaisher's case and his own supposed case, for he was one of the majority of two-thirds of Council who voted for the confirmation of the award of the medal to myself. It appears, however, that he did not consent to be named in an opposition list, only introducing the reference to his character I suppose for the sake of slily prodding Mr. Glaisher. He knew nothing of the opposition list or its anonymous authors, and it was against his express wish that he was inserted in it as secretary—but how he came to express a wish to persons unknown, about a matter of which he knew nothing, deponent saith not.

Now, Sir, though I was not one of the anonymous authors of the opposition list, nor knew till the very last either what that list was to be, or that it would be printed, I took a good deal of interest in the election of two names which appeared in it, viz., Mr. Ranyard as Secretary, and Capt. Noble as member of Council. I even went so far, in my anxiety to see justice done to these faithful and zealous servants of the Society, as to do what half a year ago I could not have thought it possible that I should do. I allowed my name to be added to the opposition list, as the only means of securing some few votes which I thought might be useful to Ranyard and Noble, and which I trusted might be thrown away so far as I was concerned. I voted also, and tried to secure votes for Mr. Christie as Secretary. I even went so far (and those who know me will consider this as tolerably strong evidence of the interest I took in the matter) as to call the Astronomer-Royal's attention (after I had received a circular signed by him) to the necessity of retaining Mr. Ranyard as Secretary and Capt. Noble on the Council, and the advisability of having *one* secretary acquainted with the details of observatory work, naming Mr. Christie as eminently suitable for the office. I did this without entertaining the slightest personal feeling in the matter, and most certainly with no idea that Mr. Glaisher would be displaced from the Council, where he assuredly ought to be. Indeed, I specially called the attention of the gentleman to whom rumour assigns a large share in the preparation of the opposition list to the risk which seemed to exist that, if Mr. Christie were made Secretary, Mr. Glaisher would not be elected at all, and that gentleman pointed out reasons for believing that there was no such risk. I incline to think now that I was right and he was wrong, the risk I indicated really, I believe, existing.

What "the events which occurred last year" may have been. I have no idea. I have heard of nothing, and I am told the Council minutes mention nothing of the least importance in which Mr. Ranyard and Capt. Noble have adopted a course which should trouble Mr. Christie. In fact, the only thing I can think of is their very mild and courteous opposition to Mr. Brett's theory that Venus is a burnished globe in a glass shell. But though Mr. Christie has said that he regards Mr. Brett's idea as an important contribution to science (save the mark !) he could hardly wish to turn Capt. Noble off the Council and displace Mr. Ranyard from the secretaryship because they do not agree with him on *that* point. On that principle I fancy he would reduce the Council to himself and Mr. Brett.

RICHD. A. PROCTOR.

MIRA CETI.

Sir,—It may interest some of your readers to know that this remarkable variable is now visible to the naked eye. It was last night (6th Dec.) about 5½m.

Umballa, Punjab :
Dec. 7, 1876.

Yours faithfully,
J. E. GORE.

OCCULTATIONS OF REGULUS.

Sir,—A densely clouded sky hid all view of the occultation of Regulus here this morning, as has indeed been the case with every first magnitude occultation for the past 10 years. In this case it would have been interesting to notice the effects of parallax on so short an occultation in different parts of the country. In January, 1840, Regulus was occulted for a few minutes in the S. and S.W. parts of England, to the N. and N.E. her upper limb passed below the star. It was remembered at the time that the duration of that occultation would differ very sensibly if observed from the top of a hill, and in a contiguous valley, or at the level of the sea.

The following records of the curious illusion of projection on the moon's limb, during occultations of this star, will be found in the *Monthly Notices*. The star was occulted on December 22, 1831. In the observation at Cambridge it is remarked that "all the observers saw the star hang on the limb, and in some degree projected on the disc."

In May, 1858, we were favoured with a clear sky for this phenomenon, and a moon no further than the first quarter. I witnessed it through an old ship glass carrying a magnifying power of 16, too small to show anything worthy of note. Some observers saw the star "snuffed out in a moment" on this occasion. Others had a different impression. Mr. Worthington, at Crumpsall, says : "At the reappearance, rather more than half the disc of the star was projected instantaneously from behind the moon, but about three-quarters of a second elapsed before the whole of the disc became visible."

A curious observation of an occultation of Regulus occurred on July 30, 1821. It was observed by Mr. Rumker and Sir Thos. Brisbane, at sea, with a comet sweeper, five miles from Cape Frio. The sun had just set, and the star described a very small chord behind the moon's disc. Both observers saw the star within the moon's limb so long that they exchanged the telescope several times to "admire in turns the singular phenomenon."

Upton Helions Rectory: S. J. JOHNSON.
Jan. 3, 1877.

AURORA BOREALIS.

During a watch for shooting stars on the evening of Jan. 6, between 8h. 30m. and 9h. 30m., a bright aurora was seen here, and a more marked display than any I have witnessed since October 4, 1874. They have been very infrequent and feeble during the last two years.

Between 8·30 and 9, cumulus clouds more or less obscured the northern sky, but a strong glow, as of twilight or moon rise (though the light seemed more condensed and stronger on the horizon), became apparent through fissures. At 9 the sky had cleared, and a bright band of white light was seen lying parallel along the horizon, just below the stars ε, γ and δ Cygni, γ and β Draconis, and η Ursæ, extending in its whole length from ζ Cygni to Cor Caroli (upwards of 80 degrees) and slightly above *α* Lyræ, then shining very brightly from a clear space below. This band was about 3 or 4° broad, and on either side of it the sky was very dark and clear. At 9.5 the light became more diffused, spreading itself upwards over the stars of Cygnus and Draco's head, and at 9.10 was detached in the centre, resolving itself into two luminous clouds, one condensed just under γ Cygni, the other below η Ursæ. Then the latter faded rapidly while another luminosity formed upon γ and β Draconis. These, however, became quite faint before 9.15 when the aurora again assumed the form of a bright white band stretching horizontally along, and some 4° broad, though slightly higher this time and projected upon the stars of Cygnus and Draco, before noted, in position almost parallel along the horizon. This second band, scarcely as intense as the preceding, sloped downwards to the right under Cor Caroli, where it appeared to meet the visible horizon, and a few minutes sufficed to render it nearly extinct though it had been rapidly undergoing variations before. At 9.30 the phenomenon had wholly gone, and at 10 p.m. the sky became cloudy and a driving rain fell during the whole night afterwards. There were no vertical streamers, in fact, this aurora exhibited itself solely as luminous patches or bands, lying more or less parallel to the horizon, giving the impression of bright moonlight reflected on films of cirro-stratus clouds. During the time I saw it the wind was blowing a gale from S.W., barometer about 29 inches (corrected). I had observed frequent lightning between 8 and 9 p.m., proceeding chiefly from cumuli in N.W., and from other parts violent gales and thunderstorms, with much rain and hail, are reported on the same day and night, and I mention the fact merely as of contemporary occurrence.

Bristol: January 7, 1877. W. F. DENNING.

[Mr. Glaisher's meteorological report for quarter ending December, 1876, confirms Mr. Denning's note on the unfrequent occurrence of the auroræ of late.]

ASTRONOMICAL OCCURRENCES FOR MARCH, 1877.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Regulus
Thur	1	8 39 8 47 3	Near approach of χ Virginis (5) Near approach of B.A.C. 4259 (6) Conjunction of Jupiter and Mars 0° 32′ S.			11 22·4
Fri	2	16 23 17 20 16 58	Occultation of 83 Virginis (6) Reappearance of ditto Near approach of 85 Virginis (6)	2nd Tr. I. 2nd Sh. E.	16 58 17 19	11 18·5
Sat	3		Sidereal Time at Mean Noon 22h. 45m. 27·66s.	1st Oc. R.	18 16	11 14·5
Sun	4		Sun's Meridian Passage 11m. 51·76s. after Mean Noon	1st Tr. E	15 33	11 10·6
Mon	5					11 6·7
Tues	6	10 0	☾ Moon's Last Quarter			11 2·7
Wed	7	14 21	Conjunction of Moon and Jupiter 5° 23′ N. Conjunction of Moon and Mars 4° 36′ N.	3rd Ec. D.	18 31 49	10 58·8
Thur	8					10 54·9
Fri	9			2nd Sh. I.	17 11	10 50·9
Sat	10			1st Ec. D.	16 43 11	10 47·0
Sun	11		Saturn's Ring: Major axis=35″·12 Minor axis=2″·63	1st Tr. I. 3rd Tr. E. 1st Sh. E. 2nd Oc. R. 1st Tr. E.	15 13 16 0 16 12 17 12 17 28	10 43·1
Mon	12	21	Conjunction of Moon and Mercury 1° 35′ S.			10 39·1
Tues	13	10 16	Conjunction of Moon and Venus 1° 24′ S. Conjunction of Moon and Jupiter, 2° 4′ S.			10 35·2
Wed	14	14 53	● New Moon			10 31·3
Thur	15	22	Conjunction of Saturn and Venus, 0° 20′ N. Illuminated portion of disc of Venus=0·974 Illuminated portion of disc of Mars=0·887			10 27·3
Fri	16		Sidereal Time at Mean Noon, 23h. 36m. 42·86s.			10 23·4

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m.
Sat	17		Sun's Meridian Passage 8m. 26·19s. after Mean Noon			10 19·5
Sun	18	10 30 22	Occultation of μ Arietis (5½) Conjunction of Saturn and Mercury 0° 37′ S.	1st Sh. I. 1st Tr. I.	15 50 17 6	10 15·5
Mon	19			1st Oc. R.	16 35	10 11·6
Tues	20			2nd Tr. E.	14 21	10 7·7
Wed	21	10 20 10 51	Occultation of B.A.C. 1746 (6½) Reappearance of ditto			10 3·7
Thur	22	1 9 10	☽ Moon's First Quarter Jupiter at quadrature with Sun			9 59·8
Fri	23	11 23 12 10	Occultation of κ Geminorum (3½) Reappearance of ditto			9 55·9
Sat	24					9 51·9
Sun	25	11 33 15	Near approach of Uranus Conjunction of Venus and Mercury 0° 41′ S.	3rd Sh. I. 2nd Ec. D. 1st Sh. I.	16 16 17 7 18 17 43	9 48·0
Mon	26	5 49 6 40 8 23 9 25 10 6 11 11	Occultation of 45 Leonis (6) Reappearance of ditto Occultation of ρ Leonis (4) Reappearance of ditto Occultation of 49 Leonis (6) Reappearance of ditto	1st Ec. D.	14 58 43	9 44·1
Tues	27			2nd Tr. I. 2nd Sh. E. 1st Sh. E. 1st Tr. E. 2nd Tr. E.	14 12 14 26 14 26 15 43 16 58	9 40·1
Wed	28	17 48	○ Full Moon			9 36·2
Thur	29			3rd Oc. R.	14 6	9 32·3
Fri	30					9 28·3
Sat	31	17 9 17 56	Occultation of B.A.C. 4984 (6) Reappearance of ditto Saturn's Ring: Major axis=35″·46 Minor axis=1″·99			9 24·4
APRIL *Sun*	1	11 53	Occultation reappearance of 4 Scorpii (6)			9 20·5

THE PLANETS FOR MARCH.

At Transit over the Meridian of Greenwich.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	21 19 19	S. 17 0	6″·1	22 38·0
	9th	22 4 44	S. 13 57	5″·6	22 51·9
	17th	22 53 29	S. 9 32	5″·3	23 9·0
	25th	23 45 18	S. 3 50	5″·0	23 29·2
Venus ...	1st	21 51 56	S. 14 8	10″·1	23 10·5
	9th	22 30 20	S. 10 47	10″·2	23 17·4
	17th	23 7 48	S. 7 7½	10″·0	23 23·3
	25th	23 44 33	S, 3 16	9″·9	23 28·5
Mars ...	1st	18 2 1	S. 23 35½	7″·2	19 21·2
	9th	18 24 44	S. 23 37	7″·6	19 12·4
	17th	18 47 28	S. 23 27	8″·0	19 3·6
	25th	19 9 48	S. 23 6	8″·4	18 54·5
Jupiter ...	1st	18 0 32	S. 23 2½	33″·5	19 19·8
	9th	18 4 54	S 23 2½	34″·3	18 52·7
	17th	18 8 37	S. 23 2	34″·9	18 24·9
	25th	18 11 36	S. 23 1½	36″·1	17 56·5
Uranus ...	2nd	9 37 52	N. 14 57½	4″·2	10 54·6
	22nd	9 35 6	N. 15 11	4″·2	9 33·2

Mercury rises three-quarters of an hour before the sun at the beginning of the month, the interval decreasing.

Venus rises about half-an-hour before the sun, the interval decreasing.

Mars rises three hours and a quarter before the sun, at the beginning of the month, the interval slightly decreasing.

Jupiter rises about three hours and a quarter before the sun on the 1st, the interval increasing to about four hours on the last day.

Uranus is well situated for observation.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN MARCH, 1877.

By W. R. Birt, F.R.A.S., F.M.S.

In the list for January, 1877, *ante* p. 23, we gave a series of objects near the north border of Fracastorius, which would form a suitable line to compare with and check the base line given in November.

Mr Simms has kindly furnished us with a continuation of the catalogue of Area IV. Bρ, from which we shall extract his observations of four of these objects. The following are the numbers in the Synopsis with the corresponding numbers in the catalogue:

Synopsis,	XV.,	XXIV.,	XVI.,	XXVI.,	XXVII.,	XXIX.
Area IV. Bρ	23	21	24	20	18	16.

IV. Bρ 21. A ridge branching off from the north extremity of the west wall of Fracastorius, in a north-westwardly direction; its northern extremity is abrupt; it was observed August 8, 1876; it is not found in Dennett's drawings.

IV. Bρ 20. A small craterlet in the angular point included between the southern extremity of IV. Bρ 8 (the ridge to Rosse, XXV. of the Synopsis) and the N.E. extremity of a ridge or range of mountains lying in a N.E. and S.W. direction, and joining the northern extremity of the west wall of Fracastorius.

IV. Bρ 18. A small craterlet situated on the exterior margin of the old northern border of Fracastorius.

This portion of the northern border has only been visible to me [Mr. Simms] under a high illumination, and then only by its bright tint. I have never detected the slightest shadow here, even when the east wall was on the terminator.

The craterlet IV. Bρ 18 is situated about midway between the angular point of the old northern border and the junction of IV. Bρ 8 with the border of Fracastorius. This craterlet when last seen appears to be surrounded by a uniformly smooth surface.

IV. Bρ 16. A minute craterlet N.W. of the shallow crater XI. of the Synopsis (see list for August, 1876, No. 164, p. 191), seen by Simms once only on August 8, 1876.

In the list for December, 1876, p. 297, No. 168, a craterlet IV. Bρ 19 is given between B. and M.'s *b*, IV. Bρ 18, and the three-peaked mountain IV. Bρ 9. Simms describes it thus: "A small craterlet S.E. of Rosse. It seems to me to be the largest of the craterlets mentioned in this list, it was the first which I detected in the neighbourhood."

There appears to be a very curious difference, almost amounting to a *discrepancy*, between the drawings of Mr. Simms and Mr. Dennett. Simms places the craterlet IV. Bρ 19 *considerably* to the *east* of the ridge IV. Bρ 8, extending from the northern extremity of the west wall of Fracastorius to Rosse. Dennett places it, December 4, 13.0. 1876, *on the ridge*. Simms gives the ridge convex towards the *west*, Dennett convex towards the *east*. This state of things is inexplicable. Fracastorius lying between 30° and 35° of west longitude, libration could not produce such a difference. Simms appears to have observed the region very carefully, and speaks of the surface to the west of the ridge lying at a *higher* level than that to the east, the determination of these points is important as bearing on selenological considerations.

Errata.—List for January, 1877, p. 23, second paragraph, line 11, for *XXVIII.* read *XXVII.* Line 13, insert "object" between "sixth" and "in."

SATELLITES OF URANUS.

The angle of position *p*°. of the major axes and the dimensions of the semi-axes *a* and *b* of the apparent orbits are in March :—

0h. G.M.T.		Ariel.		Umbriel.		Titania.		Oberon.	
1877.	*p*°.	*a.*	*b.*	*a.*	*b.*	*a.*	*b.*	*a.*	*b.*
March	°	″	″	″	″	″	″	″	″
1.	10.01	15.18	6.08	21.14	8.46	34.68	13.88	46.38	18.57
6.	9.95	15.15	6.11	21.11	8.52	34.62	13.97	46.29	18.68
11.	9.89	15.12	6.14	21.06	8.56	34.55	14.04	46.20	18.78
16.	9.83	15.08	6.17	21.01	8.60	34.46	14.10	46.08	18.85
21.	9.78	15.04	6.19	20.95	8.63	34.36	14.15	45.95	18.92
26.	9.73	14.99	6.21	20.88	8.65	34.25	14.18	45.80	18.96
31.	9.69	14.94	6.22	20.81	8.66	34.13	14.20	45.65	18.99

In these apparent orbits the satellites move so that the angles of position decrease, or they move from their greatest northern elongations N. (in posit. p°) through their inferior conjunctions (in posit. p° — 90°) to their greatest southern elongation S. (in posit. p° + 180°), and through their superior conjunctions (in posit. p° + 90°) round to N. again. The nearest hours of their passing their greatest elongations, and, in the cases of Titania and Oberon, also their conjunctions are found in the following list:—

1877.	Ariel G. M. T. h.			Umbriel. G. M. T. h.			Titania. G. M. T. h.			Oberon. G. M. T. h.	
Mar. 1.	9	S.	Mar. 1.	14	S.	Mar. 2.	13	inf.	Feb. 28.	12	inf.
2.	15	N.	3.	15	N.	4.	17	S.	Mar. 3.	21	S.
3.	21	S.	5.	17	S.	6.	21	sup.	7.	5	sup.
5.	4	N.	7.	19	N.	9.	1	N.	10.	14	N.
6.	10	S.	9.	21	S.	11.	6	inf.	13.	23	inf.
7.	16	N.	11.	22	N.	13.	10	S.	17.	8	S.
8.	22	S.	14.	0	S.	15.	14	sup.	20.	17	sup.
10.	5	N.	16.	2	N.	17.	18	N.	24.	1	N.
11.	11	S.	18.	4	S.	19.	23	inf.	27.	10	inf.
12.	17	N.	20.	5	N.	22.	3	S.	30.	19	S.
13.	23	S.	22.	7	S.	24.	7	sup.	Apr 3.	4	sup.
15.	6	N.	24.	9	N.	26.	11	N.	6.	13	N.
16.	12	S.	26.	10	S.	28.	16	inf.			
17.	18	N.	28.	12	N.	30.	20	S.			
19.	0	S.	30.	14	S.	Apr. 2.	0	sup.			
20.	7	N.	Apr. 1.	16	N.						
21.	13	S.									
22.	19	N.									
24.	1	S.									
25.	8	N.									
26.	14	S.									
27.	20	N.									
29.	2	S.									
30.	9	N.									
31.	15	S.									

A. M.

DISCOVERY OF NEW PLANETS.

The discovery of a new planet is given in *Astronomische Nachrichten*, No. 2,118. (172) February 5, 1877, by M. Borelly, at Marseilles.

5th Feb. 12h. 7m. 54·3s., Marseilles, T.

R. A. 10h. 35m. 36 45s. N. P. D. 80° 30′ 55″·83. M. 12.

A *new* planet (?) was observed at Toulouse by M. Perrotin, on the 10th of January, 1877, magnitude 12; and another *new* planet (?) observed at Marseilles by M. Borelly, on the 13th January, 1877, magnitude 11 – 12.

Books received.—"Handbook of Descriptive Astronomy. By George F. Chambers, F.R A.S. Third Edition. Oxford: Clarendon Press. 1877."—"Präparation von Emulsionplatten bei Tageslicht. Von Dr. Oswald Lohse. 1877."—"Les Terres du Ciel, description astronomique-physique, &c. Par Camille Flammarion. Paris: Didier & Co. 1877."

TO CORRESPONDENTS.

We are obliged to postpone our list of subscribers, and several interesting articles, from want of space.

All Letters requiring an answer must enclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or *penny* postage stamps, but the Editor will not be liable for loss in transmission.

Post Office Orders for *the Editor* are to be made payable to John C. Jackson, at Lower Clapton, London, E.

The Astronomical Register.

No. 172. APRIL. 1877.

ROYAL ASTRONOMICAL SOCIETY.

Session 1877—78.

The First Meeting of the new Session, March 9th, 1877.

Prof. Arthur Cayley, M.A., LL.D., F.R.S., Sadlerian Professor of Mathematics, Cambridge, *Vice-President,* in the Chair.

Secretaries—Mr. Ranyard and Mr. Glaisher.

The minutes of the preceding meeting were read and confirmed.

Ninety-four presents were announced, amongst which, as meriting special attention, Mr. Glaisher mentioned a copy of the new enlarged edition of Mr. Chambers' "Astronomy," and two volumes by Dr. Booth on new geometrical methods. Dr. Booth holds one of the livings of the Society and has dedicated these volumes to the Council.

John Rand Capron, Esq., Guildown, Guildford, Surrey.
Dr. William Evans, Llanerchymedd, Anglesey.
Frank McClean, Esq., M.A., 23, Great George Street, Westminster.
R. Meldola, Esq., F.C.S., 21, John Street, Bedford Row.

were duly elected Fellows of the Society.

J. Sidney White, Esq., and
G. F. Hardy, Esq.,

were duly admitted Fellows of the Society.

Mr. Barrow: Perhaps you will permit me, sir, to convey a piece of intelligence to the Fellows which I think they will be glad to hear. We had some little conversation privately at the last two meetings with regard to a misfortune which the Society laboured under, which we understood was to be permanent, namely, that we were not for the future to have the Greenwich publications distributed to us; and as I happened to be acquainted with a gentleman at the Treasury, who might be able to throw some light on the subject, I wrote to him, and I should like to read you a few words from a letter which I have received from

Sir William Hart Dyke: "With respect to your letter on the subject of the gratuitous distribution of the Greenwich publications, I understand the alteration made by the Treasury has been to place the distribution of these works in the hands of the Astronomer-Royal; and the object of this has not at all been to suspend the gratuitous distribution, but to place the responsibility on an official representing H.M. Government." I have not read the whole of the letter. Perhaps the Astronomer-Royal will tell us whether we are still to have them.

Prof. Cayley: There is no change, I understand, except a change for the better.

The Astronomer-Royal: I have to reply, sir, that the statement which has been received is perfectly correct. By an arrangement of the Treasury, the distribution of the Greenwich observations is placed in my hands. In representing to the Treasury the advantage that, according to the usage of Government offices, there would be in confining the distribution to one officer, I took care to give a list of the persons to whom, with the authorization of the Council of this Society, distribution has been made hitherto. Of course it will be necessary from time to time to revise the lists of distribution, and I can say for myself that the Royal Astronomical Society will have as good a share of the distribution as it has had heretofore.

Prof. Cayley: We are glad to hear that satisfactory statement from the Astronomer-Royal.

The Astronomer-Royal: On the application of any members of the Royal Astronomical Society, the same attention will be paid by me as formerly by the Council of the Royal Astronomical Society.

Prof. Cayley: The Astronomer-Royal has two communications to make to the Society, and I will ask him to favour us with them now.

The Astronomer-Royal: With your permission, there are two points upon which I wish to address the Society. The first of them relates to a subject with which the Society in general have become familiar since the publication a few days since of the report of the last general meeting, namely, the search which it is proposed to make for the Intra-Mercurial planet. I need not say that it is a matter which has been investigated almost entirely by my honoured friend M. Le Verrier. He has brought together all the materials—very imperfect ones, as he does not himself disguise—which gave a suspicion of the existence of a planet between Mercury and the sun; and upon putting these together with a degree of skill which few could imitate, he has come to the conclusion that there is a possibility that if there be such a planet it will cross the sun's disc about March 22nd, or it may be on March 21st, or on the following day, the 23rd. On the strength of this suspicion he has circulated a dispatch to most of the governments and observatories concerned in astronomy, requesting their observation of the sun carefully on March 22nd in particular, and on March 21st and 23rd also. Now, in order to do what I can to promote these observations, I will in the first place state what is the general nature of the demands made upon us. The planet will not occupy perhaps more than two or three hours in passing over the sun's disc, perhaps hardly so much. In that case it is useless to confine our observations to this part of Europe. We may perhaps observe the sun during nearly twelve hours of those days, but there remain as many hours in which we cannot observe the sun, and it is of the utmost importance to make provision for observations on the different parts of the earth on which the sun may be observed during the intermediate hours. Now, we cannot do very much in this matter. I have taken care that telegrams should be sent, one to India, three to Australia, and two to New Zealand, which is a very important station because it is in longitude almost immediately opposite to ours; and I have also requested that telegrams may be sent to Washington and Santiago de Chili; and there is another station in America I wish we could insure observations being taken from, namely, San Francisco. Taking the stations as I have given them to you, it will be seen that the gap is over the Pacific Ocean. I have hopes that M. Struve will get observations made, as far as he can, in Eastern Siberia, and possibly in Japan, or some of the islands beyond; and if we can get all those that I have mentioned, we should get the whole circumference of the earth pretty well filled up; and if people at every one of those stations would take observations of the sun at intervals not exceeding two hours, or as much oftener as they think fit, during the three days I have mentioned, we should go a good way towards the solution of this problem. There is another thing to be considered and it is this: in looking at the sun in a telescope you may skip over various parts of it. It has been well laid down by some of the metaphysicians of the last century that all you see at any one instant as you look at an object is the *minimum visibile,* and that the rest of the surface is quite lost, the most sensitive part of the eye is not at the moment directed towards it; and thus observers may fail to examine sufficiently all parts of the sun. I am free to say that that doctrine assisted me very much in understanding some of the queer red spots and prominences said to arise suddenly in the observations of the various eclipses of the sun. Now, if you can observe the sun with a telescope, you do a good thing, and if you can catch the planet on it so much the better; but you are always under the risk of not having surveyed every part of the disc with the proper minute attention; but if you can get a photograph of the sun then you have a record which you can survey at your leisure, and it will be difficult for a trifling little spot to escape you. I should wish, therefore, in the interest of science, that photoheliographs were infinitely distributed over the globe, but I am sorry to say there are not many that will help us. I have been endeavouring to do my best, but if there are persons in England, members of this Society, who can take care that as regards their part nothing shall be neglected, and if some of them are blessed with a photoheliograph apparatus, let them have photographs taken at frequent intervals.

Without saying positively that I believe, or disbelieve, in the existence of the planet, although M. Le Verrier is so confident, yet I do mean to say that this is an opportunity which ought not to be neglected by anybody who professes to take an interest in the progress of planetary astronomy. So, gentlemen, on March 21st, 22nd, and 23rd let the sun be observed from sun rising to sun setting, at intervals not greater than two hours. That is the first task I have set you.

The other subject upon which I propose to speak to the Society is one in which I have for many years taken an interest; and that is with reference to another determination of the parallax of the sun. I believe that we have done all that mortal men can do, with regard to the late transit of Venus. I will take up a volume which I have brought containing the *Monthly Notices* of this Society for the year 1857. In the May of that year I gave an oral address with regard to the means then available for correcting our measures of the sun's distance during the next 25 years. Of those 25 years a large portion are gone since 1857. I pointed to the years which are available for determining the parallax by the transit of Venus, and also pointed to another opportunity which would occur in the year 1877, and I will read what I said then of this method of determining the sun's distance: "It is a method by observing the displacement of Mars in right ascension when he is far east of the meridian and far west of the meridian as seen at a single observatory." And here after giving certain numbers, I point out the large amount of parallax that would be exhibited by this method at stations near to the equator. After this I say, "To this it is to be added that the method is attended with no expense whatever; that the observations which are compared are made with the same telescope by the same observer, on the same series of observations, and there is none of the wearying labour and trouble which are inseparable from observations requiring distant connection, and that the observer is supported by the feeling that his own unassisted observations will give the means for deciding one of the most important questions in astronomy," and to this it was added, speaking in the third person, "The Astronomer-Royal expressed his opinion that this method is the best of all." I hold that opinion still, gentlemen, and knowing now even better than I did at that time the uncertainties of various kinds which attach to the observations of the transit of Venus, I believe the best method is that of observing, at individual stations not requiring to be connected with others, the difference of right ascension between Mars and the stars in its neighbourhood in the morning and evening of the same day. I have endeavoured, as is well known to members, to bring this matter before you on several occasions. Those who have looked at the late numbers of the *Monthly Notices* will find a chart of mine for giving facilities for observations of the parallax of Mars in this coming proximity in 1877. Now, in order to show the value of this opposition in 1877 for the purpose, I have here (communicated by a friend) the distances of Mars from the earth, at the oppositions of various years, expressed in the unit of the earth's distance from the sun. 1877, ·37; 1879, ·48; 1881-2, ·60; 1884, ·67; 1886, ·76, and then the figures go on diminishing till in 1892 when it comes to ·38, which in respect to distance nearly agrees with 1877, but in other respects is less fortunate; therefore, suppose we lose the opportunity this year, we shall have to go another 15 years before we get another opportunity which is approximately equal to this year, but which is not absolutely equal to this. I think, gentlemen, that under the circumstances it behoves us as astronomers to do the best we can to get satisfactory observations in the present year. The opposition will take place from August to October, and during that time every day may give a series of observations which are comparable, in my belief, with the observations of the transit of Venus. Its cost is a mere trifle compared with that of the transit of Venus. The transit of Venus has cost the British Government more than £39,000, and I suppose has cost about as much to each of the other countries concerned. Now, the expense of observing the opposition of Mars is a mere trifle compared with this, and I will occupy a few moments in describing to you the different ways in which the change of the apparent right ascension of Mars between morning and evening can be observed. When I wrote this account I had no hope of making any observations of Mars, except with an equatoreal, and I took care to insist that with an equatoreal extraordinary firmness would be necessary, especially with an equatoreal such as the German manufacturers make, and also with those which the best English manufacturers have made. Everything would depend upon firmness, and if this was attained the difference of the right ascension of the stars near it could be observed with very great accuracy; but in the course of the last few months the opportunity has presented itself to me and to science of making a better observation, I believe, than I then contemplated, by means of the heliometer. There are very few persons in England who are acquainted with the use of the heliometer. It has been thought available only for such things as measuring the diameters of planets and the distances of double stars and things of that sort, but it has advantages such as are peculiar to double-image-micrometers, and it may be used with a total independence of the firmness of the instrument. The observation is made in a moment, both images move together. Everybody knows how a midshipman with a sextant which seems to quiver, can get a good observation of the altitude of the sun and the distance of a star from the moon; and so it may be with this instrument on a different scale of accuracy; and subject to a good deal of quivering, it may be made to get perfect measures of the distance of Mars from the stars, and such elements as will absolutely fix the apparent place of Mars at the time. An opportunity of using the heliometer for this purpose has presented itself, for which I was unprepared, and for which I am indebted to Mr. D. Gill. Mr. Gill was entrusted by Lord Lindsay with the use of his heliometer. It was used for a long time by him in observing Juno's place with respect to stars, in which task he has not only acquired experience in the adjustments of the instrument, which is so desirable, but he has had the opportunity of constant practice, and apparently he will enter upon such an investigation as this with advantages that none else has ever possessed. Now, Mr. Gill proposes to devote his own time to this purpose; and in selecting a station for these observations he has fixed upon two places to choose from, St. Helena and the Island of Ascension. He proposes to take these observations at an expense not exceeding £500, and in case of the expense exceeding £500 it shall not be chargeable to anybody, but be borne by himself. (Hear, hear.) I do not know how irregular it may be to bring a matter of this kind before the Society at an ordinary meeting, but I think it desirable to pick up as far as we can the feelings of the Society in general upon a point of this kind. Here is an opportunity occurring in a few months whereby at an expense of £500 we can do what £100,000 has not done before, as I firmly believe. Now, I think, sir, that this is an occasion on which the Royal Astronomical Society may well step in with its funds. (Hear, hear.) I speak of its funds in contradistinction to those of the

Government, to which some allusion has been made, because the Government has been very liberal (hear, hear), and has felt a great deal of strain put upon it by the expenses which have already been incurred in other expeditions. When I look upon what has been expended in observations of eclipses, and on behalf of the transit of Venus, I am grateful to the Government for what it has done (hear, hear.), and for that reason I am very cautious indeed in suggesting any further application to them; but this Society is not without funds which have been supplied for astronomical purposes, and I think they might be well employed for this particular purpose. Mr. De La Rue suggests that the Carrington bequest might be available. Whatever may be done, I thought it desirable to bring this before the Society as a public body, and to gather their sense on the matter generally before making any more formal application to the Council or any other body. My feeling upon this matter is very strong. The thing ought to be done, and I hope the Society will do it. (Hear, hear.) If the Society will not do it, I hope the members of the Society will do it, and all I can say is that there is £20 for the purpose; but I hope on all accounts for the credit of the Society that it will not be necessary to appeal to anything like that. I believe from the suggestions Mr. De La Rue has just made there are funds available, and I hope the sense of the Society may be expressed in such a way as will justify the Council of the Society in devoting a portion of their funds to this purpose.

Prof. Cayley: Before we go further perhaps I may mention that there are several copies here of the sweeping ephemeris of the Intra-Mercurial planet compiled by M. Le Verrier, from the 10th to the 31st March, if any members care to take them. I believe Mr. Gill is here, and I will call on him to speak on the subject.

Mr. Gill: After the statement of the Astronomer-Royal it must seem superfluous for me to say anything. In the first place his appeal to the generosity of the Society, places me in the position of a beggar were I to touch the question of ways and means ("No, No"), and that is a position I do not wish to assume. In the second place I can add nothing to the clear and able statement you have just heard of the geometrical conditions of the question; and if I must say something it is to call attention to some of the technicalities of the proposed method of observation, by which I think entire immunity from systematic error is secured. The first test to be applied to any series of observations, to judge of their accuracy and the value of the result, is not the apparent agreement between the various results *inter se*, but the arrangement of the method in such a way that the causes of error produce opposite effects on the desired result and become thus eliminated. Now, of all micrometers the heliometer is the most accurate for the measurement of absolute distances within the limit of its range, but accurate as it is there are systematic errors due to various causes which attach to the results when it is used otherwise than as an instrument of interpolation between two fixed points, and it is only when so used that the highest capabilities of the instrument are developed. Thus, if the distance and position angle of the planet relative to a star are measured in the evening and morning, we have the means (the motion of the planet being known) to compute the parallax. Such an observation is liable to the objections, "How do you know that, from temperature or some other cause, the scale value has not changed between the evening and morning observations, and how do you know that the zero of the position-circle is the same?" There are, indeed, very many sources of error, all small, but all of a nature which can be hardly be computed and allowed for, as they are not a function of the temperature. It so happens that when the instrument is all of one uniform temperature, the temperature co-efficient is almost insensible, because the tube, the focal length of the object-glass, and the scale have nearly the same co-efficients of expansion, and in these circumstances these relative proportions are unaffected by temperature. This case, however, is seldom realized in practice, hence the relative proportions are changed and the scale value is altered. Again, the observer's eye is in reality part of the eye-piece, in practice the eye-piece is unchanged, but the actual state of the accommodation of the observer's eye in observing is affected by the physical condition of the observer, and thus a very minute, though very sensible, change of scale value is produced.

No calculation can take account of such errors. But now let us look at the method when the heliometer is employed as an instrument of interpolation. Suppose the planet situated nearly in the line joining two stars, and nearly mid-way between them, and let us suppose the positions of these stars to be determined with considerable accuracy, such an accuracy that the distance between them may be used to determine the scale value of the instrument for the short distance traversed by the planet between evening and morning.

Let the distance and the position angle of the planet from each star be measured in the evening and morning, taking care to arrange the observations symmetrically, that is, comparing the planet with star *a*, then star *b*, then star *b* again, and finally with star *a*. Then having reduced these measures on an assumed approximate value of the scale, we obtain from each star an observed distance which we shall call O. With assumed places of the stars and the tabular place of the planet (corrected for parallax on an assumed value of the parallax), we compute the theoretical distances which we shall call C. But the assumed places of the stars are supposed to be so approximate, that they may be taken as absolute, and their small error taken as the error of the planet. If we now compute—

f_i the effect on the distance of increasing the A.R. of the planet 1″
f_{ii} „ „ „ „ Decl. „ by 1″
f_{iii} „ „ of increasing the parallax one per cent.

Each measure of distance will give us an equation of the form—

$$f_i \, \Delta\alpha + f_{ii} \, \Delta\delta + f_{iii} \, \Delta\pi + (C - O) = 0$$

This equation is so far precisely the form of equation laid down by the Astronomer-Royal as the best for combining the various modes of observing the transit of Venus. When $\Delta\alpha$ $\Delta\delta$ are the errors of the planet in A.R. and Decl. referred to the stars, and $\Delta\pi$ the per cent. in which the assumed parallax is in error.

Now, we have only to introduce another term to express the error of the assumed scale value, if we put

z = the error of the assumed scale value in 1000″.
n = the distance of which the unit is 1000″.

Then we have for the complete equations resulting from the evening observations two equations of the form—

$$f_i \, \Delta\alpha + f_{ii} \, \Delta\delta + f_{iii} \, \Delta\pi + n \, z + (C - O) = 0$$

because we have arranged the observations symmetrically, z may be assumed the same for both stars. We shall from the morning observations also have two similar equations, but we must not assume z to be the same, we shall call it z^1. We have now five unknown quantities and four equations, but we get a fifth from the position angles, the mean of these being also free from systematic error, because the effect of a change in the zero of the position-circle between evening and morning produces opposite effects on the angles relative to the two stars.

The solution of these five equations then gives the five unknown quantities—

$$\Delta\alpha \quad \Delta\delta \quad \Delta\pi \quad z \text{ and } z^1$$

in which in the final combination we suppose $\Delta\alpha$ and $\Delta\delta$ to be different, when different stars of comparison are employed. No sensible errors arise from the supposition that the star places are exactly known.

In the proposed observations it is intended to triangulate the stars of comparison by heliometer measures, and combine these measures with all meridian and other determinations that can be procured. The stars of comparison will be from 5000″ to 10000″ apart—the motion of the planet between evening and morning is seldom more than 250″, therefore the effect of even 1″ error in the assumed distance of the comparison stars (and that is a very improbably large error) would only be to produce an error of $\frac{1}{20}''$ to $\frac{1}{40}''$ of arc in the measured motion—and this error being of an entirely accidental character would be eliminated in a long series of observations.

There only remains to be considered such errors as would arise from abnormal refraction—the effect of such abnormal refraction is to change the apparent distance of the stars of comparison, and will therefore be eliminated as part of our terms z and z^1.

The instrumental errors of scale and screw have in the case of Lord Lindsay's heliometer been investigated with great care, indeed, it is only a question of the patience and skill of the observer to determine these with any desired accuracy, but it is a very capital feature of the proposed observations that in a good series, from the employment of many different divisions and many different parts of the screw, these produce no systematic effect on the ultimate result.

I may state in conclusion, that the results derived from the Juno observations confirm most strongly these statements, nor do any of them differ so much as do the values of the solar parallax, which have been derived from one and the same series of observations of the transit of Venus.

Mr. De La Rue: The subject of the proposed observations, which have just been described, has come under my notice in another place, in consequence of an application for aid out of the funds of £4,000 which the Royal Society has been called upon by the Government to recommend the distribution of. In order to facilitate its distribution the subjects for recommendation were divided into three classes, and referred to sub-committees. £14,000 was applied for, and we had £4,000 to distribute. The sub-committees were not in some cases able to recommend the total sum asked for, and they felt that to recommend half might stultify the object in view, and the sub-committee concerned did not, partly on that account, feel itself warranted in recommending so large a sum as £500 for the observation of Mars. At the same time the committee felt strongly the urgency of the object, and a recommendation will go to the Council of the Royal Society to ask that body to apply to the Government, but it is quite possible, as the Astronomer-Royal has said, that it may fall through, that is to say, the Government having voted large sums already, may not feel warranted in appropriating even £500 out of the public money. But most certainly I agree with the Astronomer-Royal, that these observations must be carried out. (Hear, hear.) They cannot be allowed to drop through, and we must always recollect that the £500 asked for is only a portion of the contribution required, and had it not been for the existence of the heliometer and the kindness of Lord Lindsay, the proposal could not have been brought forward at all. I am one of those who think that, as regards the money left by the late Mr. Carrington, we could not do anything so much in accordance with his views as to appropriate a part of his legacy to so important a series of observations as is now proposed. That of course is a matter for the consideration of the Council. The Astronomer-Royal has suggested whether this sum might not be raised amongst ourselves, and he has stated that he is willing to contribute. All I wish to say now is that, if the funds are to be raised in that way, my name may be put down for £100. (Hear, hear.)

Admiral Ommanney suggested that the greatest publicity should be given to the remarks of the Astronomer-Royal, and that a report should be sent to the *Times* by the Secretary, as well as to the scientific journals.

Mr. Brett: It may not be known that official reports of the proceedings of this Society are contributed by the Secretary to the various periodicals that deal with science. They are officially sent out by the Secretaries.

Mr. Ranyard: I think Mr. Brett is mistaken. There is no official report of the meetings.

The Astronomer-Royal: A great many members of the Society see the *Astronomical Register*, and I am happy to bear my testimony to the general fidelity of its reports.

Mr. De La Rue: I must also bear my testimony to the accuracy of the *Astronomical Register* reports.

Mr. Ranyard: I have a short paper by Mr. T. W. Backhouse *On the nomenclature of meteor radiant points*. Mr. Backhouse says, "It appears to me that the naming of radiant points should not be left to individual astronomers. I would suggest that their nomenclature should always be referred to the British Association Committee, and whoever wishes to give a new name should communicate with the committee."

I have another short paper from the Astronomer-Royal, rather an unreadable paper, on an alteration in his numerical lunar theory. The paper is entirely algebraical.

I have another paper which is hardly suitable for reading, from Mr. Marth. It is one of his valuable ephemerides for the physical observation of Jupiter for the whole of this year. It gives the angle of position of Jupiter's axis with respect to the north point, and the Jovicentric latitude and longitude of the centre of the disc for every five days during the year, and there is a column giving the annual parallax, by which I believe Mr. Marth means the angle between the earth and sun as seen from Jupiter. This column will be very valuable for finding the position of the shadows of satellites and clouds upon the planet. No doubt the paper will be found very useful.

Mr. Ranyard said he had also a paper by Senor Arcimis, of Cadiz, *On the visibility of the unilluminated side of Venus.* Senor Arcimis thought that he had seen the dark part of the disc of the planet shining with a pale ashy grey light against the black sky. The dark part of the planet appeared spherical, and when the bright limb was hidden by a wire he still thought he could, without doubt, perceive the dark limb.

Capt. Noble: Did Senor Arcimis imagine he saw the dark limb of Venus, light or black, against the sky?

Mr. Ranyard: He says he saw it shining with an ashy grey light, and it appeared spherical as seen upon the sky.

Captain Abney was then called upon to read a paper *On the effect of a star's rotation on its spectrum.* He said he had asked Mr. Huggins privately at the last meeting if he had considered the effect that would be produced by the rotation of a star on its spectrum, and had received a negative answer. It had struck him that as each part of a star's spectrum is composed of light derived from all parts of the star's disc, the light coming from the advancing limb of a star would show dark lines slightly shifted in respect to the dark lines in the spectrum of the light of the receding limb, and that owing to the overlapping of these dark lines from the two limbs, the lines as seen in the spectrum of the whole star's light would appear broader and shaded off at their edges. He thought that, from this expansion of the lines, the size and rate of rotation of the stellar masses might be determined.

Mr. Ranyard: There are other physical causes which may give rise to the broadening of the lines in the spectra of stars. In the case of the sun, the shift of the lines, due to the advance of one limb and the recession of the other limb, is a very small quantity indeed as compared with the space between the D lines in the solar spectrum. As far as I remember, the advance of the equatorial limb of the sun towards us, is only about 1·25 miles per second, and even if we suppose the stellar masses to be ten times as great as our own sun, the rate of advance or recession of their limbs to or from us would only be small compared with the velocity of the storms which we know to be taking place in the solar envelopes.

When we look at the sun with an integrating spectroscope, we get all the effects of the movements from and towards us summed together, and even if one observes with an ordinary analyzing spectroscope, the lines, as every one knows who is used to looking at the sun's spectrum, are not quite sharp, the reason possibly is, that even in the small part of the sun's image covered by the slit, there are motions from and towards us, the velocity of these solar storms we know sometimes amounts to as much as 200 and 300 miles per second, which would be a quantity very large indeed, even compared with the recess and advance of the limbs of a star ten times the diameter of our own sun. The winging or shading of the edges of the lines in some of the stellar spectra is, I believe, very remarkable, but there are physical causes which may give rise to this broadening of the lines. We know that here in our laboratories, when the pressure upon a gas is increased, the lines become broadened out into bands, the theoretical explanation of which is, as you know, that when a gas is pressed together, and the impacts between the molecules of the gas become more frequent, the jar in the swing of the parts of the molecules caused by the impacts, takes an appreciable time to run down, and so the lines appear winged or broadened out into nebulous bands. I think, therefore, that we cannot assume that, because the stellar lines are broad, therefore the broadening is due to the advance or recession of the stellar limb.

Prof. Cayley: It seems that the theory of Capt. Abney's paper is quite correct, but that the broadening of the lines may be due to other causes.

Mr. Ranyard: Yes, that is what I mean.

Mr. Christie: I think there is a little confusion in Mr. Ranyard's remarks with regard to two sorts of lines of different nature. The lines of the stars are absorption lines, and the lines which Mr. Ranyard has spoken of, and which show the existence of storms, are bright lines. The dark lines are not affected by the motion of the gas from which they come.

Mr. Ranyard: If the absorbing gas is moving relatively to us, the dark lines will be shifted just as much as bright lines would be.

Mr. Christie: The absorption lines will only be affected by the motion of the absorbing medium. In the case of the sun, the solar atmosphere, which gives rise to the dark lines, is not in motion, and but little absorption takes place in the layer above, where the storms are. I cannot bear out Mr. Ranyard's assertion that all the lines are winged. I think that very likely the appearance of winging has its origin in the spectroscope. It is a common fault to see the lines winged; but, of course, there are great difficulties in the way of applying Capt. Abney's idea to practice, though it is very probable they may be overcome as improvements are made, but, as Mr. Ranyard has pointed out, the fact that pressure comes in is of great importance. The remark of Capt. Abney as to the obliteration of the finer absorption lines seems to me to be capable of leading to some definite results, because these finer lines, which come from the metallic spectra, are, I fancy, not affected by pressure or temperature to the same extent, and we may deduce more certain conclusions from them.

Capt. Abney: A great deal that Mr. Ranyard has said I agree with, but there is much that I do not agree with. Mr. Christie has referred to some of the points in which I would join issue with Mr. Ranyard. I shall only touch on one part of the secretary's remarks. As regards the rotation of the star, he has assumed that, if the star were ten times as big as the sun, the rotation would be only ten times that of our sun; but I do not think that follows. You may have the star rotating considerably faster; there is no necessity for a particular standard of rotation, and therefore you might have it rotating 50 times more quickly. I am glad to say I believe I have been successful in being able to show the effect of the rotation of the sun by photographing the spectrum, and, surely, if one can do that with the sun, with a little more experience one ought to be able to photograph the rotation of stars.

Mr. Ranyard: You would, I think, never be able to eliminate the effects of storms and pressure.

Capt. Abney: Never is a very long word, and I should not like to say so.

Mr. Gibbs: Rotation will always be in action, but storms only occasionally; for instance, now there are no prominences.

Mr. Ranyard: I think that at no time shall we find the surface of the chromosphere flat. With regard to what Mr. Christie said, if there are these great storms in parts of the chromosphere, from which we get the bright lines, we can hardly conceive that in the lower and reversing parts of the chromosphere there are not also disturbances. Gas moving at such enormous velocities must, of course, give rise to currents on all sides.

Mr. De La Rue: There is one point to which attention requires to be directed, and that is the great difference between the rate of rotation at different latitudes of the sun. The storms blow in all directions, but the rotation is only in one direction; therefore we have the means of correcting and eliminating the displacement due to the storms.

Mr. Gill: I should like to hear a little more as to how Capt.

Abney is quite sure that he has no systematic error with regard to the shift of the lines he has photographed. Mr. Christie has made some experiments on the shift of the lines due to the rotation of the sun I also made some measurements, and at first I found most beautiful and consistent results. However, I found that they were not worth anything at all, because when the sun was allowed to shine on one side of the slit it caused a great displacement one way, and when it shone on the other side it caused a great displacement in the other direction. It was only after a great deal of labour, and by complicated arrangements of screws, that anything could be done. I should be glad to hear from Capt. Abney if there was not something of the kind in his case; but I have no doubt Capt. Abney has good reason for what he has said.

Capt. Abney: I did not like to say too much about my experiments, or to be quite sure there is no systematic error; but as far as I know there is none. Until I have further investigated the matter, I should be sorry to bring it before thesociety. (Hear, hear.)

Mr. Ranyard said he had a paper from Mr. Stone "On apparent brightness as an indication of distance in stellar masses." After reading a part of the paper, Mr. Ranyard said: It seems to me that the reasoning assumes that the stars are equally distributed in all directions, and that would be contrary to the assumptions usually made that they are more thickly distributed about the plane of the milky way.

Mr. Brett: Are you reading the paper, or is that your own commentary?

Mr. Ranyard: The paper is rather a long one, and, as it is getting late, I have only given the substance of the paper. What I was saying is, of course, commentary. (Hear, hear.)

Mr. Penrose was then called upon to read a paper on *Observations of Occultation of Regulus and of the Total Eclipse of the Moon.* The moon was at first hidden by clouds at its reappearance, the sky was clear, and a star was seen as a bright point just within the limb of the moon. It could not have failed to be remarked. The light upon the eclipsed moon was brighter on the northern and southern parts of the disc. He presumed that this might be due to the prevalence of clouds in some parts of the earth's atmosphere, and perhaps also specular reflection from the northern and southern ice-caps of our globe. With respect to the total lunar eclipse, although no value for the determination of longitude appears to be set on them in works on navigation, he thought that the edges of the earth's shadow were so sharp that they would give a useful result. The colours seen on the moon during the late eclipses were especially remarkable. He showed a drawing, in which he had tried to represent them.

Capt. Noble: Everybody must have noticed the remarkable red appearance of the moon at the time of totality. I don't know whether any one here examined this light spectroscopically, but I did. I have one of Mr. Browning's small direct-vision spectroscopes; and at the time when the moon was remarkably red, I examined the light, and I was exceedingly struck with the result. I should have expected to have found a spectrum cut off at the violet end, and tolerably conspicuous at the red end; but I was astonished to and that it extended for a considerable distance into the violet, find hardly began much before D. I tried a rough experiment by striking a lucifer, so as to obtain the D line for comparison, and it certainly seemed to me that the spectrum of this light, which looked a deep red, began on the left of D, going from the red, and nearly all the spectrum was given down in the blue and violet. It struck me as remarkable, because the contrast between what one saw with the eye and what one saw spectroscopically seemed inexplicable.

Mr. ——: I looked at it with a small Browning's spectroscope, but I saw the red and yellow. I could not see the blue end at all, and I was obliged to use an open slit to see anything at all.

Capt. Noble: When I did get a spectrum, I saw there was a lavender-coloured light of a dim sort, and when I struck the lucifer match, and threw the light of the match on the comparison prism, I got my D line somewhere about the beginning of the light, at the red end. I do not understand what it means.

Mr. Glaisher then read a paper by M. de Gasparis *on Kepler's problem* The method proposed in the paper was somewhat similar to that known to mathematicians as Horner's method—viz., the numerical value of a certain quantity *n* had to be determined; a first approximation gave this value, by an interpolation, to the nearest tenth of a unit, and a second approximation gave the hundredths and thousandths, the solution of Kepler's problem thus obtained being true to within a few seconds. Mr. Glaisher remarked that this solution of Kepler's problem was very different to that which M. de Gasparis had given in the *Astronomische Nachrichten* for 1857, and which, in effect, consisted of a double-entry table giving *e* as tabular result for M and E as arguments. The method of the present paper was very simple and elegant. Mr. Glaisher added that he had himself been working at an elliptic-function solution of Kepler's problem, which had been recently proposed by Dr. Gyldén, and that he hoped to communicate the results of the investigation to the

Society at its next meeting. Dr. Gyldén's method differed *toto cœlo* from that of M. de Gasparis, as while the former depended upon an intricate branch of mathematical theory which was of recent creation, and had scarcely hitherto been applied to any of the questions of astronomy, the latter rested upon the most elementary mathematics, and seemed to be a new and valuable application of the most simple and well-known principles to the solution of this time-worn problem. As a practical solution, that of M. de Gasparis seemed to be excellent, and he thought it would probably be found more convenient in its use than that proposed by Dr. Gyldén.

Capt. Tupman was called upon to read a paper entitled, *Corrections to the Nautical Almanac places of the Moon for longitude observations in connection with the transit of Venus*, 1874. The Astronomer-Royal, he said, had thought it advisable to communicate to the Society the corrections that had been derived from all the observations we have been able to collect. The observations we have used were those made with the transit circle and with the altazimuth at Greenwich, at Paris, Königsberg, and Strasburg. The observations at Washington were not available at that time. After drawing a curve to represent the corrections, we were anxious to see how it would agree with Professor Newcomb's theory on the subject, in giving the theoretical value for the corrections of the lunar ephemeris, and I am sorry to say it does not agree at all. Therefore, we have relied solely on observations, and have been obliged to leave Professor Newcomb's corrections out of consideration. The longitudes of Honolulu, Kerguelen, and Rodriguez depend entirely on the moon, and it is a matter of extreme importance to get the errors of the moon's place accurately.

Mr. Gill: This is a matter that no man can speak feelingly upon who has not suffered from longitude on the brain, as men connected with the transit of Venus have done. A short time ago Capt. Tupman supplied me with the results of his determination of the corrections of the moon's tabular place. It was necessary to get them to confirm the longitudes of Lord Lindsay's station. Mauritius had been connected with Aden by chronometers, and Aden with Berlin by telegraph. When the occultation observations in Mauritius were computed, the difference between the two results was less than $\frac{4}{10}$ths of a second of time, a result which is extremely gratifying, and is due to the great accuracy with which Captain Tupman has determined the mean corrections. I think the result is very satisfactory; indeed, it is smaller than the probable error of the two determinations.

Mr. Neison: I understand that in these curves the black line is the difference between Hansen's tables and the observed place of the moon, whereas Professor Newcomb has introduced certain corrections to Hansen's tables, and from the corrected places he has discovered his new inequality which would make a great difference. (No, no.) Professor Newcomb specified in his paper more than one correction to be introduced into Hansen's tables, and after those corrections had been taken into consideration there then remain certain differences which he explained by his inequality.

Captain Tupman: Professor Newcomb has printed a table of daily corrections of the *Nautical Almanac.* The introduction of his fresh terms was in order to meet a discrepancy between the observations and the calculated places. He has given tabular corrections which do not satisfy the observations. Dr. Auwers made out, from a number of observations of the moon, that Professor Newcomb was in error with regard to the mean longitude of the moon, about 1½″ of arc, but if any one will look at these curves he will see that no assumption of error of mean longitude will satisfy the conditions.

Mr. Ranyard read a paper by Father Perry *On the total eclipse of the moon of the 27th February last.* He had noticed the differences in the intensity of the shadow on different parts of the moon. Mr. Ranyard said: I had an opportunity of observing the eclipse with Mr. Webb, at Cheltenham, and we both remarked the bright line round the edge of the dark moon, like the bright line round the limb of the dark part of the moon when seen in the young moon's arms, and I should like to know the cause of this bright line along the moon's limb.

Dr. Royston Pigott read a paper *On photographing solar transits by means of his starlit transit eyepiece.* He said: This is a paper to suggest to the Society whether there may be any utility in photographing the disc of the sun in transit. If we may rely on the *Nautical Almanac,* diameters of the sun are given within a $\frac{1}{100}$th part of a second.* The method I propose is simply to place a screen at a given distance from the telescopic focus, so that by an intermediate lens a good image may be formed on the screen at a given distance. If you suppose a film of silver sufficiently thick to obscure most of the solar rays, but not sufficient to spoil the outline of the sun, you might get, by means of an instantaneous photograph, an outline image of the sun, with bright lines across it, corresponding to the series of bars which would be cut on the plate of silver, at perfectly equal distances, measurable by the microscope within $\frac{1}{10,000}$th of an inch. The instrument might be arranged for a flash, and the brilliancy of the bars is so great that there would be no difficulty in taking an instantaneous photograph of them. But there are some practical difficulties which I have considered. One is irradiation. I have presumed that the radiation of the sun's limb and the radiation of a given bar would be almost identical. One thing seems perfectly clear, that if you were to use a film of silver sufficiently dim to extinguish the irradiation, but not sufficient to extinguish the limb of the sun, then you would find a photograph of the exact outline of the sun with each particular bar of the photograph where it was brilliant, at almost constant intervals within the limb. Whether this method might have been applicable to more accurate photographs of the transit of Venus, is a question open to discussion, but for my own part I bring it before the Society with great diffidence, having no practical knowledge of photography.

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* [Note by Dr. Royston Pigott]. Time seconds in passage of the semi-diameter over the meridian.

Captain Noble: I should like to have a grumble. When I was a young man, and a comet or anything of that sort appeared in the heavens, a little notice by post was sent to every known observatory in the kingdom: but on the 9th February there was a comet discovered by Borelli, and but for that admirable publication, the *English Mechanic,* I should never have heard a syllable of it. Had I had notice I might have found it. It would not be very difficult to send a little fly-sheet or a post-card to each of the observatories.

Mr. Christie: There is a difficulty about that comet. One observation was made and then another, and it was a long time before the third could be made; its motion was so rapid it was utterly impossible to indicate its place. A few post-cards were sent round to some of the principal observatories, but there was no time to send them round to all, especially as it is difficult to know who is inclined to observe comets. In this instance, however, the great difficulty would have been to indicate its place. You could not tell within 10° where it was to be.

Mr. Neison: Mr. Christie is referring to Greenwich, I think this should be done by the Society.

Mr. Christie: I am making an excuse for the Society not doing it. I presume it could not come any sooner to the Society than it came to us.

Mr. Ranyard: We received the notice of its place the day after it was published in *Nature.*

The following papers were also announced and partly read:—

Prof. C. V. Zenger: *The Katadioptric Aplanate.*

C. Todd: *Observations of the phenomena of Jupiter's satellites at Adelaide, South Australia.*

A. T. Arcimis: *Observations of occultations of stars by the moon, and phenomena of Jupiter's satellites, made at Cadiz in the year* 1876.

S. W. Burnham: *Note on the double star* H. I. 62.

Dr. T. R. Robinson: *Note on the relative space-penetrating power of reflectors and refractors.*

J. Tebbutt: *On the magnitude of η Argus.*

Prof. J. Newcomb: *On observations of contacts of the limb of Venus or Mercury with that of the Sun.*

J. Ennis: *On solar physics.*

Prof. R. S. Ball: *Note on a transformation of Lagrange's equations of motion in general co-ordinates, which is convenient in physical astronomy.*

G. Knott: *On the variable star* U *Geminorum.*

E. Neison: *On the lunar perturbations arising from the planet Jupiter.*

J. R. Hind: *On two occultations of planets by the moon, observed by the Chinese.*

The meeting adjourned at a quarter past ten o'clock till Friday, April the 13th.

Since the meeting, the Astronomer-Royal has received a communication from M. Struve, stating that "the Russian observatories are all informed by letter of the date on which Le Verrier expects that a transit of the Intra-Mercurial planet may happen. Besides this, telegraphic despatches have been sent to Taschkent, Irkutsk, and to my brother Charles in Japan. Unfortunately, our regular observers at Wladiwostok and Tomsk are at this moment absent on leave from their stations."

1877, March 16.

[We have just received from the Astronomer-Royal a report that no trace of the planet was seen at Greenwich on March 21, 22 or 23. The weather was only partially favorable.]

THE ECLIPSE AND THE LUNAR AURORA.

Prof. Dorna, Director of the Astronomical Observatory of Turin, has sent the following letter to the Abate Serpieri, Director of the Observatory of Urbino :—"To you who are the author of the most recent and the most important publication on the zodiacal light, and who believe that it is a terrestrial phenomenon, and of the same nature as the aurora borealis, I suggest an idea that occurred to me when observing the eclipse of the moon last evening, which is that, during the eclipse, it is possible we may have perceived a *lunar aurora.* In fact, how are we to explain those rapid changes—and so vivid—of reddish light, and those sudden centres of light in different points of the face of the moon (which had the appearance, as seen in my telescope, as if volcanos were kindled and extinguished) unless we attribute this to the effects of electric excitement; for I do not find adequate reasons in the diffusion, diffraction, and refraction of the solar light in the earth's cone of shadow. For my part, it is one or both of the following: 1. An excitement of the sun on the earth, which produced luminous auroral effects, by which the cone of shadow traversed by the moon was illumined by auroral light and fluorescence of volatile particles, light which the face of the moon reflected; and it is certain that, during the eclipse, I observed a zodiacal light so marked as I never saw even in the tropical regions of India; and the axis (an arc of a great circle) of the pyramid of that light, a little before eight o'clock, was directed between Aldebaran and the Pleiades, and prolonged passed near the moon, which, as well as the vertex of the pyramid itself, was very near the ecliptic. 2. The electric excitement produced in the earth, and manifested in the zodiacal light, had place, perhaps, in a greater degree in the moon itself, and manifested itself in those, I might almost call them, "Flames of light."—From *L'Opinione Nazionale*, March 3. Florence.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

THE STAR LALANDE 42360.

Sir,—Not far from the place of Schmidt's new star in Cygnus is the star Lalande 42360, rated as 7m. in 1752, but which is not now visible with a powerful binocular. On the evening of the 21st January I observed with 3-inch refractor a small star about 11m. very close to the position given by Lalande (closely *preceding* his No. 42383, 8m.). If Lalande's estimate of its magnitude was at all correct, the star must certainly be variable. The fact of its having been registered by Lalande at all, is a proof of its having been brighter at the epoch of his observations, as his catalogue contains no star fainter than 10m.

The star Lalande 42306, rated 8m. by both Lalande and Harding, is now only 10m. or 10½m. This may also be variable. These stars should be watched by observers when examining the vicinity of Schmidt's new star.

Umballa, Punjab: Your obedient servant,
Jan. 29, 1877. J. E. GORE.

STAR IN CANIS MINOR POSSIBLY VARIABLE.

Sir,—About 1° 20′ *south following* the 5m. star of Canis Minoris (6 Flamsteed) Harding shows a 6m. star which is not in Lalande's catalogue. On the night of Feb. 4, I found this star only 7½m., being then less than Lalande 14720 (7m.), but brighter than Lalande 14726 (8m.)

About 30′ *n. p.* Harding underlines a 7m. star, which is also missing in Lalande's catalogue. This star is now about 7m., and a little brighter than Harding's 6m. star above referred to.

Umballa Punjab: Yours faithfully,
Feb. 5, 1877. J. E. GORE.

ECLIPSE OF THE MOON.

Sir,—I beg to send you a few short lines of remarks upon the total lunar eclipse which was visible here in a clear and unclouded sky. During the totality the *effective* light of the moon was absolutely nothing, eight small stars—seven of them being very small—were at once distinguished, all being within a diameter of the moon from her limb, by means of an object glass of 1·25 inches aperture. The colour of the moon's disc was of a coppery red, but far from being deep, and still less so through the telescope than when seen by unaided vision. At the middle of the eclipse the central parts of the lunar disc were especially dark, and the markings upon it were barely to be discerned ; the circumference, however, for a breadth of 90″, was much brighter, and thus gave somewhat of the aspect of an annular eclipse. The deepest part of the earth's shadow was very marked even to the unassisted eye, and could readily be traced moving across the disc, clearly indicating to one unacquainted with it the direction of the moon's motion. When the eclipse became partial the three gradations of shade of the uneclipsed portion of the penumbral, and of the more dark or "total" part, were most clearly defined, the bright crests being more distinct than in any lunar eclipse I have seen in England. The phenomenon was seen to great advantage from the Monte Pincio, the sky during totality appearing like a black vault studded with brilliant stars, traversed by the milky way, and by the zodiacal light, which stretched to a distance of 9° to 11° beyond the Pleiades.

I am yours obediently,

Rome: Feb. 27, 1877. JOHN T. BARBER.

METEORS, &c.

Sir,—The following is a list of meteorological phenomena observed here last year :—

	Halos.		Thunder or	Days on which		
	Solar.	Lunar.	Lightning.	Rain or Snow fell.	Meteors.	
January	2	1	0	14	4	No regular watch kept.
February	13	2	0	22	0	
March	1	3	1	22	1	
April	6	2	3	18	8	
May	7	0	2	12	5	
June	7	0	3	16	3	
July	5	0	4	13	108	
August	3	1	2	12	157	
September	8	5	8	21	102	
October	5	2	3	18	126	
November	5	3	3	20	150	
December	5	1	3	25	185	
	67	20	32	213	849	

I also saw 4 displays of parhelia, 10 rainbows, 28 red sunsets, and on a few evenings saw pink rays from the sun after sunset.

On 5 nights I suspected auroral light in the north, but no streamers were seen. Of the 849 meteors, 191 had trains or left streaks, and 71 were of the first magnitude. The greatest number of the latter were seen in August and December, October and November having the fewest large meteors.

Yours truly,

Writtle: January 8, 1877. H. CORDER.

THE ZODIACAL LIGHT.

This light was seen here on the 2nd, 4th, 12th, 16th, 18th, 19th, 26th, 27th and 28th of February, and on the 14th and 19th of this month. It was very finely seen on the last named date.

JOSEPH GLEDHILL, F.R.A.S., &c.

Mr. Edward Crossley's Observatory,
Bermerside, Halifax: March 20, 1877.

THE INTRA-MERCURIAL PLANET.

We are informed by the Astronomer-Royal that "preparations are made under British influence for the search for the small planet, in America, in India, and in Australia."

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN APRIL, 1877.

By W. R. Birt, F.R.A.S., F.M.S.

In No. 23 of the *Astronomical Register*, November, 1864, p. 264, Mr. Herbert Ingall called attention to a spot on the moon of about 6° or 7° of brightness and little to the west of Picard, which he saw on May 15, 1864, and again on October 16, 1864. In the following number, December, 1864, pp. 295, 296, a careful description of the spot is inserted. In an article in the *English Mechanic*, January 12, 1877, p. 425, entitled, "Telescopic Work for Moonlight Evenings," mention is made of this spot, and also that on December 12, 1864. Knott discovered two minute craterlets on its site, 1 C μ 2 and 1 C μ 5. When at its greatest apparent size it was generally estimated as double the size of Picard. At intervals during the years 1873, 1874 and 1875, Mr. Herbert Sadler observed this spot. On May 1, 1873, he recorded it as "west of Picard P ± 270° D 15″. 'hard to define, seemed hazy.'" On July 28, 1873, he writes, "small, not much bigger than Picard." On April 20, 1874, he says, "very little whiter than adjacent markings, not whitest part of Mare," but on the 21st, one day later, he describes it as "about the brightest spot on floor of Mare." On February 10, 1875, he records it as "perhaps the brightest on floor, very elongated, and misty," and on March 10, 1875, as "very faint, with craterlet in it seen as a black point," probably 1 C μ 2 or 5. On the next day, March 11, Mr. Sadler described it as "almost circular, the craterlet very well seen, even with a power of 80; there seemed to be a very faint white marking, slightly less in size than the spot N.W. of it. The spot certainly brighter than yesterday (the 10th). Distance of spot from Picard ± 1½ diameters of Picard from W. to E. Size of spot ⅓rd of Picard."

The observations of 1873—4—5, compared with those of an earlier date, are conclusive as to apparent variability, both as regards size and brilliancy, and in connection therewith further observations are very desirable.

The eastern portion of the Mare Crisium has lately been very carefully observed. We hope in our next list to compare the observations with those of Schröter, made in 1788 and 1789, and illustrated in *Tafels* XXXIII, XXXIV and XXXV of his Fragments.

4, St. Scholastica's Retreat,
Kenninghall Road, Clapton, E.

ASTRONOMICAL OCCURRENCES FOR APRIL, 1877.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. β Leonis
Sun	1	11 53	Occultation reappearance of 4 Scorpii (6) Sidereal Time at Mean Noon 0h. 39m. 47·72s.			11 1·2
Mon	2		Sun's Meridian Passage 3m. 33·72s. after Mean Noon	1st Ec. D.	16 52 19	10 57·3
Tues	3			1st Sh. I. 2nd Sh. I. 1st Tr. I. 1st Sh. E. 2nd Tr. I. 2nd Sh. E.	14 4 14 17 15 20 16 19 16 46 17 2	10 53·3
Wed	4	3	Conjunction of Moon and Jupiter 5° 11′ N.	1st Oc. R.	14 50	10 49·4
Thur	5	4 29 18	☾ Moon's Last Quarter Conjunction of Moon and Mars 3° 1′ N.	2nd Oc. R. 3rd Oc. D.	14 8 15 19	10 45·5
Fri	6	1	Inferior conjunction of Mercury and Sun			10 41·5
Sat	7					10 37·6
Sun	8					10 33·7
Mon	9					10 29·7
Tues	10	6	Conjunction of Moon and Saturn, 2° 31′ S.	1st Sh. I. 2nd Sh. I. 1st Tr. I.	15 57 16 53 17 11	10 25·8
Wed	11			1st Ec. D. 1st Oc. R.	13 14 25 16 41	10 21·9
Thur	12	21	Conjunction of Moon and Venus 4° 58′ S.	1st Tr. E. 3rd Ec. D. 1st Oc. R. 3rd Ec. R.	13 54 14 20 2 16 37 16 44 37	10 17·9
Fri	13	5 49	● New Moon			10 14·0
Sat	14	2	Conjunction of Moon and Mercury 3° 57′ S.			10 10·1
Sun	15		Illuminated portion of disc of Venus=0·996 Illuminated portion of disc of Mars=0·868			10 6·1
Mon	16	11 14	Near approach of χ^1 Tauri (5½)			10 2·2

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. β Leonis
Tues	17		Sidereal Time at Mean Noon, 1h. 42m. 52·59s.			9 58·3
Wed	18		Sun's Meridian Passage 0m. 44·35s. before Mean Noon	**1st** Ec. D.	15 8 5	9 54·3
Thur	19			**1st** Tr. I. **2nd** Ec. D. **1st** Sh. E. **1st** Tr. E.	13 28 14 3 3 14 34 15 43	9 50·4
Fri	20	7 37	☽ Moon's First Quarter Saturn's Ring: Major axis=36″·12 Minor axis=1″·41	**1st** Oc. R.	12 59	9 46·5
Sat	21	7 20	Near approach of 83 Cancri (6)	**2nd** Tr. E.	13 49	9 42·5
Sun	22					9 38·6
Mon	23			**3rd** Tr. I. **3rd** Tr. E.	12 35 15 16	9 34·7
Tues	24	11 15	Mars at quadrature with Sun Conjunction of Sun and Neptune			9 30·7
Wed	25	12 38	Near approach of ψ Virginis (5)			9 26·8
Thur	26	12 3 12 47 12 23	Occultation of 85 Virginis (6) Reappearance of ditto Near approach of 83 Virginis (6)	**1st** Sh. I. **1st** Tr. I. **1st** Sh. E. **1st** Ec. D.	14 12 15 16 16 27 16 36 16	9 22·9
Fri	27	4 35	○ Full Moon	**1st** Oc. R.	14 48	9 18·9
Sat	28			**1st** Tr. E **1st** Tr. I. **1st** Sh. E. **1st** Tr. E.	11 59 13 29 14 10 16 16	9 15·0
Sun	29	14 22 15 34	Occultation of τ Scorpii (3½) Reappearance of ditto			9 11·1
Mon	30			**3rd** Sh. I. **3rd** Sh. E. **3rd** Tr. I.	12 3 14 41 16 11	9 7·1
MAY Tues	1	12	Conjunction of Moon and Jupiter			9 3·2
Wed	2					8 59·3

THE PLANETS FOR APRIL.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	0 33 48	N. 2 5½	5″·0	23 50·1
	9th	1 25 47	N. 8 35	5″·0	0 14·4
	17th	2 27 28	N.15 41½	5″·6	0 44·5
	25th	3 23 59	N.20 56½	6″·6	1 9·4
Venus ...	1st	0 16 23	N. 0 12	9″·8	23 32·7
	9th	0 52 44	N. 4 10	9″·8	23 37·5
	17th	1 29 22	N. 8 3	9″·6	23 42·6
	25th	2 6 37	N.11 44½	8″·6	23 48·3
Mars ...	1st	19 29 13	S. 22 39	8″·8	18 46·3
	9th	19 51 7	S. 21 59	9″·3	18 36·7
	17th	20 12 36	S. 21 11	9″·9	18 26·7
	25th	20 33 34	S. 20 15	10″·3	18 16·1
Jupiter ...	1st	18 13 35	S. 23 1½	36″·8	17 30·9
	9th	18 15 4	S 23 1	37″·8	17 0·9
	17th	18 15 43	S. 23 1	38″·8	16 30·1
	25th	18 15 29	S. 23 1½	39″·6	15 58·4
Uranus ...	3rd	9 33 54	N.15 16½	4″·2	8 44·8
	19th	9 33 1	N.15 20	4″·2	7 41·0

Mercury is favourably situated for observation after sunset, towards the middle of the month, setting more than two hours after sunset on the last day.

Venus is too near the sun to be well observed.

Mars rises about three hours after midnight on the 1st, the interval slightly decreasing.

Jupiter rises an hour and a half after midnight at the beginning of the month, the interval decreasing. On the last day he rises about half an hour before midnight.

SATELLITES OF URANUS.

p° angle of position of the major axes, *a* and *b* semi-axes of the apparent ellipses, which the satellites describe. *Vide* page 77.

0h. G.M.T.		Ariel.		Umbriel.		Titania.		Oberon.	
1877.	*p*°.	*a.*	*b.*	*a.*	*b.*	*a.*	*b.*	*a.*	*b.*
April	°	″	″	″	″	″	″	″	″
0.	9·69	14·94	6·22	20·81	8·66	34·13	14·20	45·65	18·99
5.	9·66	14·88	6·22	20·73	8·66	34·01	14·20	45 48	18·99
10.	9·63	14·82	6·21	20·65	8·65	33·88	14·19	45·30	18·98
15.	9·61	14·76	6·20	20·57	8·64	33·74	14·17	45·11	18·95
20.	9·60	14.70	6·18	20·48	8·62	33·59	14·13	44·92	18·90
25.	9·59	14.64	6·16	20·39	8·59	33·44	14·08	44·72	18·83
30.	9·59	14.57	6·13	20·30	8·55	33·30	14·02	44·52	18·75
May									
5.	9·60	14·50	6·10	20·21	8·50	33·14	13·94	44·32	18·64

The nearest hours of the satellites passing their greatest elongations (N. and S.), and, in the cases of Titania and Oberon, also their conjunctions, are found in the following list:—

Ariel			Umbriel.			Titania.			Oberon.		
G. M. T.			G. M. T.			G. M. T.			G. M. T.		
1877.	h.			h.			h.			h.	
April 1.	21	N.	April 1.	16	N.	April 2.	0	sup.	April 3.	4	sup.
3.	3	S.	3.	17	S.	4.	4	N.	6.	13	N.
4.	10	N.	5.	19	N.	6.	9	inf.	9.	21	inf.
5.	16	S.	7.	21	S.	8.	13	S.	13.	6	S.
6.	22	N.	9.	23	N.	10.	17	sup.	16.	15	sup.
8.	4	S.	12.	0	S.	12.	21	N.	20.	0	N.
9.	11	N.	14.	2	N.	15.	2	inf.	23.	8	inf.
10.	17	S.	16.	4	S.	17.	6	S.	26.	17	S.
11.	23	N.	18.	6	N.	19.	10	sup.	30.	2	sup.
13.	5	S.	20.	7	S.	21.	14	N.	May 3.	11	N.
14.	12	N.	22.	9	N.	23.	19	inf.	6.	20	inf.
15.	18	S.	24.	11	S.	25.	23	S.	10.	4	S.
17.	0	N.	26.	13	N.	28.	3	sup.	Ariel.		
18.	6	S.	28.	14	S.	30.	7	N.	May 2.	3	N.
19.	13	N.	30.	16	N.	May 2.	12	inf.	3.	9	S.
20.	19	S.	May 2.	18	S.	4.	16	S.	4.	15	N.
22.	1	N.	4.	19	N.	6.	20	sup.	5.	22	S.
23.	7	S.	6.	21	S.	9.	0	N.	7.	4	N
24.	13	N.	8.	23	N.				8.	10	S.
25.	20	S.							9.	16	N.
27.	2	N.									
28	8	S.									
29.	14	N.									
30.	21	S.									

A. M.

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF JUPITER.

Greenwich Noon.	Angle of position of ♃'s axis.	Longitude of ♃'s central meridian.		Latitude of Earth above ♃'s equator.	Equatoreal diameter.
1877.	°	°	diff.	°	"
Mar. 31	357·72	104·8	°	—2·31	38·71
—			870·6		
April 1	357·69	255·4	·6		38·83
2	·66	46·0	·6		38·96
3	·63	196·6	·7		39·08
4	·61	347·3	·6		39·21
5	·59	137·9	·6	—2·31	39·33
6	·57	288·5	·7		39·46
7	·55	79·2	·6		39·58
—					
8	357·53	229·8	870·7		39·71
9	·52	20·5	·6		39·83
10	·51	171·1	·7	—2·30	39·96
11	·50	321·8	·6		40·09
12	·49	112·4	·7		40·21
13	·48	263·1	·6		40·34
14	·47	53·7	·7		40·46
—					
15	357·46	204·4	870·6	—2·30	40·59
16	·45	355·0	·7		40·72
17	·45	145·7	·7		40·84
18	·44	291·4	·6		40·97

19	·44	82·0	·7		41·09
20	·44	237·7	·7	—2·29	41·22
21	·44	28·4	·7		41·35
—					
22	357·44	179·1	870·7		41·47
23	·45	329·8	·6		41·60
24	·45	120·4	·7		41·72
25	·46	271·1	·7	—2·29	41·85
26	·47	61·8	·7		41·97
27	·48	212·5	·7		42·10
28	·49	3·2	·7		42·22
—					
29	357·50	153·9	870·7		42·34
30	·52	304·6	·7	—2·29	42·46
May 1	·53	95·3			42·58

To find the longitude of central meridian for

h.	°	
13	add 111·6	to the longitude at noon.
14	147·9	
15	184·2	
16	220·5	
17	256·7	

A. M.

Books received.—"Atlas Céleste de Ch. Dieu. Rectifié, augmenté et enrichi. Par Camille Flammarion. Paris: Gauthier-Villars. 1877."—"Etudes et Lectures sur l'Astronomie. Par Camille Flammarion. Paris: Gauthier-Villars. 1876."—"Minute Structure of the Solar Photosphere. By S. P. Langley." "Comparisons of Certain Theories of Solar Structure with Observation. By S. P. Langley." "Greenwich Astronomical Results, 1874." "The Divine Order of the Universe. By the Rev. A. Clissold, M.A. London: Longmans, Green & Co. 1877." Two Reports on the Royal Observatory, Edinburgh. 1877.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Sept., 1876.
Coulcher, W. B.

To Dec., 1876.
Dallmeyer, J. H.
Fletcher, J.
Pratt, H.
Solomons, Messrs. S. & B.
Yeates & Son, Messrs.
Yeates, Miss.

To March, 1877.
Clark, Miss.
Dearden, Miss.
Heelis, J.
Wright, Rev. W. H.

To April, 1877.
Burton, C. E.

To June, 1877.
Padbury, J.

To Aug., 1877.
Fry, Miss.
Griffith, J. E.

To Sept., 1877.
Rivaz, Miss.
Bernaerts, G.
Birt, W. R.
Dancer, J. B.
Guyon, G.
Hunt, G.
Ledger, Rev. E.
Lee, J.
Lewis, R. T.
Monk, Dr.

To Dec. 1877.
Abbott, F.
Andrews, W.
Baxendell, J.
Banks, W. L.

TO CORRESPONDENTS.

We are obliged to postpone several interesting articles, from want of space.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will *at once* inform him of the fact.

All Letters requiring an answer must enclose a penny stamp.

The Astronomical Register.

No. 173. MAY. 1877.

ROYAL ASTRONOMICAL SOCIETY.

Session 1877—78.

Second Meeting of the Session, April 13th, 1877.

William Huggins, Esq., D.C.L., LL.D., &c., *President*, in the Chair.

Secretaries—Mr. Glaisher and Mr. Ranyard.

The minutes of the preceding meeting were read and confirmed.

Frederick Newton, Esq., of Hill Side House, Hornsey Lane,

was balloted for and duly elected a Fellow of the Society.

Mr. Glaisher reported that 66 presents had been received by the Society since their last meeting in March. Amongst them was a valuable double-image micrometer, which had been presented to the Society by Mr. Simms.

The President: In asking you to return your thanks to the donors of these presents, I am sure you will endorse the special vote of thanks which the Council has passed to Mr. Simms for his valuable gifts. A double-image micrometer belonging to one of the Sheepshanks instruments was out of order, and it was proposed to send it to Mr. Simms for repair, but he at once kindly offered to present to the Society a double-image micrometer of the latest form of construction. (Hear, hear.)

The President: The Fellows I am sure will be pleased to hear that, by a vote of the Council this afternoon, Mr. Gill will be enabled to carry out his proposed expedition to observe the approaching opposition of Mars. (Hear, hear.) The Council have voted £250 from the funds of the Society for the expedition, and they have further agreed to advance another £250 upon the guarantee of certain Fellows of the Society in the event of the second £250 not being forthcoming from the next year's Government grant of £4,000. There is very considerable reason to hope that it may be obtained from the Government grant of next year, and on that faith three gentlemen have engaged to guarantee the amount. The Society will, therefore, for the present advance the whole of the required sum. I am sure you will join with me in wishing Mr. Gill every possible success. (Hear, hear.) Fortunately, as you will hear from a note about to be read, Mr. Gill will be able to observe for the purpose of determining the solar parallax, not only Mars in opposition, but also three of the minor planets, Iris, Melpomene, and Ariadne. Mr. Gill, as you are aware, has already applied this method to the planet Juno, at the time of the expedition to observe the transit of Venus, and we shall have to-night a short paper by Lord Lindsay giving the result of the method applied to the planet Juno. I think it will be the best plan if I now call upon Lord Lindsay to bring his paper before the Society.

Lord Lindsay: The reductions of the observations of Juno made with the heliometer for the determination of the solar parallax being finished, it may be of interest to present some results in anticipation of the full details which will appear in Volume II. of the Dun Echt reports. The tables of the planet made use of were founded on the results of meridional observations at Greenwich and Washington, and also on the fine series of observations made by Mr. Wilson, of Cambridge, U.S. The stars of comparison were determined by Mr. Rogers. Two systems of corrections were obtained, founded upon different assumptions, greater weight being given to the Cambridge observations in the second system. The computations were made separately, on both systems, with the following results: mean solar parallax by the first method 8″·78, plus or minus, ″·041, and by the second system, 8″·77, plus or minus ·042. From the late arrival of our ship *Venus* at Mauritius, the observations could not be commenced until a week after the opposition, and in consequence of this delay one-third of the anticipated observations could not be made, and the stars of comparison could not be obtained in their most favourable position. This being so, we advance the value of the parallax obtained with caution. There is one discordant observation on November 15, which in a large series would be counterbalanced by others, but we have rejected it in the result, and the value then becomes by the first system 8″·82, and by the second 8″·81, and this we believe to be an exceedingly close approximation of the true solar parallax. At the same time the entire rejection of any observation is not advisable. At present the value is only the result of first experiments. In my paper I have laid before the Society a table showing the residuals in the equations when the errors in the star places have been eliminated. The result of the probable error of the complete comparison deduced from stars is plus or minus ·075 seconds, confirming beyond all doubt the conclusion to which we arrived in my former paper, namely, that by means of the heliometer it is possible to determine the position of the minor planets from a single star to less than $\frac{1}{10}$th of a second of arc. A near star is one whose distance is less than 1000″; a distant star would be 1000″ to 7000″ off.

The President: There are also two papers by Mr. Gill on the cognate subject of the opposition of Mars and on the observation of one of the minor planets. It would be well to take these together.

Mr. Gill: The approaching opposition of Mars is the most favourable of this century for determining the solar parallax. The horizontal parallax at opposition being within five per cent. of the difference of parallax of Sun and Venus at last transit. By observing the diurnal parallax the operations can be carried on during six weeks, under conditions of temperature and freedom from hurry and excitement, much more favourable than those attending the transit of Venus. Such observations are free from personal error. So early as 1857, the Astronomer-Royal called attention to the present opposition, and then expressed the opinion to which he adheres still, that the observations of the diurnal parallax at such an opposition presents the best means of determining the solar parallax. The method proposed by the Astronomer-Royal was to observe the difference of right ascension between the planet and neighbouring stars in the evening and morning, and with such a method in the hands of a good observer results of high value could no doubt be obtained. The success obtained in the observations of Juno led me to believe that in this method the heliometer might, with great advantage, be substituted for the ordinary equatorial. I applied in the first place to Lord Lindsay, who at once placed the instrument at my disposal. I then submitted a detailed plan of the proposed observations to the Astronomer-Royal, to the Council of the Society, to Professor Adams, to Dr. Robinson, and Mr. Hind, and I have to acknowledge the support given to the proposal by those gentlemen, as well as promises of substantial assistance from Sir George Airy, Mr. De La Rue, Mr. Proctor, and others, and also to Dr. Auwers, of Berlin, for his ready and useful advice. I beg to present to the Society the following plan: First, as to the place of observation.

In heliometer observations distances are measured with much greater accuracy than position angles, therefore we should endeavour to select stars vertically above and below the planet. A star which is vertically above or below in the evening observations, can only be vertically above or below in the morning (neglecting the planet's motion) in the one case, viz., when the observer's station is on the equator, and the declination of star and planet are ± 0°. In all other conditions we endeavour to select a station so that the planet shall be in the prime vertical, about an altitude of 30°, that is found to be the best in practice. There is no advantage in getting a larger parallax, by observing at a greater mean zenith distance, because the probable errors are increased by atmospheric disturbance, it is found best to observe altitudes from about 20° to 40°, and select stars of the same declination as the planet. In the case of Mars, this condition is realized in a latitude of about 8 degrees south. The meteorological conditions are also important. It has been suggested I should occupy the highlands of Peru, but there is a serious drawback in the difference of temperature between day and night, for it is not an uncommon thing to have a temperature of nearly 100° F. by day, and freezing by night; thus errors due to temperature might creep in. In this respect an insular position would be best, and I have selected the island of Ascension as the most suitable site. It is almost the best possible as to geographical position. The proportion of cloud is from five to six (ten being total obscuration) for the dates of observation, a proportion smaller even than Mauritius. Two to three degrees are the extreme range of temperature between 6 p.m. and 6 a.m. As to the selection of stars of observation, in making the selection a complete list was made of all the stars of the eighth magnitude, and the brighter ones in the neighbourhood of Mars. I need not read the conditions I have imposed upon myself for the selection of the stars. I have endeavoured to arrange them so that the position of the planet shall be as rigidly as possible fixed in right ascension, and I have endeavoured to fix it as well as possible in declination, also by measuring three distances instead of position angles, as was done in the Juno observations. There are many reasons why it is important that not only should the parallax of Mars be well determined, but its absolute position should be also exceedingly well fixed. I have also arranged a plan of triangulation of all the stars of comparison; the position angles and distances are contained in the tables which will be published in the *Notices* of the Society. I earnestly request the co-operation of observatories possessing good meridian instruments to determine the places of these stars. I have also included in the list of stars of comparison the star ψ' Aquarii, which was occulted by Mars Oct. 1, 1672. Mr. Hartrup, of Liverpool, is going to determine the differences of right ascension and declination of these stars of comparison. These observations will strengthen the heliometric triangulation, but do not supersede the necessity of good meridional observations.

The other paper is a note to the effect that the minor planet Ariadne will come to opposition on the 24th of July. The distance of the planet from the earth is ·83, that of the sun being unity. The declination 15 degrees south is not far from the best possible for observation at Ascension. The opposition occurs ten days before observations of Mars can be made with convenience. The planet is certainly faint, 8.8 mag., but not too faint to be measured, and comparison stars (of which a list is handed in) have been found.

In reply to the President, Mr. Gill said the other planets to be observed are Iris and Melpomene. Melpomene has a declination of two degrees north. Iris is not so favourable, unless we can secure corresponding observations in north as well as south. The declination of Iris is 24 degrees north.

The President: If there are no remarks I will ask you to return thanks to Lord Lindsay and Mr. Gill for these papers. (Applause.) Mr. Christie will explain a new form of spectroscope which he has recently designed.

Mr. Christie: I can only attempt to give a brief account of the principle of this instrument, and I cannot go very much into detail. The first thing I will begin by saying is that it is founded on the principle that if you send the rays through a prism so that the angle of incidence is less than the angle of emergence, the divergence of any two rays falling on the first surface will be increased on emerging from the second surface by virtue of the principle that as the angle of incidence increases the angle of emergence increases in a much more rapid ratio as you approach the critical angle of grazing emergence. Now, in the case of an ordinary isosceles prism, and considering the rays of one refrangibility only, say blue, so that the ordinary dispersion may be neglected, the angle between the two emergent rays will be the same as the angle between the two incident rays; but if you take a half prism, that is an isosceles prism cut in half by a plane perpendicular to its base, then for rays with a perpendicular incidence you will have the angle very much magnified when the rays emerge. Taking advantage of this principle, I have had a prism made of this form which is really the half of a direct-vision prism, that is to say there is half a prism of flint with a prism of crown cemented on to it, so that you are able to get a very great dispersion at the second surface. Now, when two slightly inclined rays of the same colour pass from the flint to the crown there is a deviation of one ray, and a much less deviation of the other, so that the angle between the two is considerably magnified. If the rays make an angle on entering, they would when they come out make an angle of 10°, and you may even increase this up to 15°. In another figure I have shown the case where you have to consider rays of different colours. Supposing two rays come from the two sodium lines, they will be nearly of the same refrangibility, but there will be a certain separation from the prism itself. The first prism will separate the sodium lines by a certain amount, and the second again. Supposing the light then falls on a half prism, the dispersion will be much magnified, but at the same time the angle between the rays coming from the two edges of the slit will be magnified, so that the breadth of the two sodium lines would be increased at the same time as their separation, and the spectrum would, therefore, not be increased in purity, although it appears more spread out. The half prism is really equivalent to a cylindrical magnifier. And one advantage is that by turning the half prism about its centre, different parts of the spectrum may be brought into the field without having to move the viewing telescope. With two such half prisms the dispersion of ten ordinary compound prisms has been obtained, and the definition is actually better, for with ten compound prisms the errors of the forty surfaces all mount up, and the lines become very ill-defined. With the half prism the breadth of the lines may be diminished by making the slit narrower, and though this reduces the light, the spectrum is still far brighter than in the other form, where the loss by absorption in the great thickness of glass used is enormous, amounting to 50 per cent. for three or four inches of glass, and to more than 99 per cent. for twenty-five inches, so that in the large Greenwich spectroscope only $\frac{1}{300}$th part of the incident light reaches the eye, and the spectrum would actually be far brighter if the prisms were much smaller. With a train of simple prisms, though the absorption would be less, it would take effect on that part of the pencil which passes through the thick part of the prisms, and thus make it virtually excentrical and much narrower. Another point is that by reversing a train of half prisms end for end, a comparatively short spectrum of great purity may be obtained; and with this form the sodium lines in any ordinary flame, or the Fraunhofer lines in the spectrum of the sun or moon may be seen without a collimator or slit.

The President: You will be disposed to return your best thanks to Mr. Christie for his very lucid explanation of this new form of spectroscope. Mr. Christie kindly showed me one of the instruments at Greenwich, and I had an opportunity of satisfying myself of the very great dispersion which the instrument gives. Unfortunately, at that time it was not in adjustment, and I was, therefore, unable to form any idea as to the definition of the instrument. Some want of definition in that instrument would arise from the small aperture. I believe Mr. Christie is now having an instrument made with larger prisms, the aperture, being larger, will-permit of greater magnifying power without loss of definition, for reasons well known, to which I need not refer.

Mr. Bidder: If I rightly follow Mr. Christie's explanation, it seems to me that the great dispersive power he speaks of is, to a certain extent, illusory. I mean in this sense: Mr. Christie explains that he gets a greatly increased spreading out of the rays of all refrangibilities, a dispersion equal to 100 ordinary prisms; but, then, the breadth of the slit is also increased in the same proportion, and if you have twenty times the dispersive power but twenty times the breadth of slit, I do not see that you get any greater purity of spectrum. It may be I have not followed Mr. Christie, but he seems to admit it, because he says you must narrow your slit to get a purer spectrum. Still, it seems to me, if that be so, the comparison between the new system and the ordinary automatic train of prisms, is not so disadvantageous, because, although it may be that in the case of the ordinary system of prisms you have a loss of light by absorption in passing through so many surfaces, and such a length of glass; on the other hand you do not need to decrease the width of the slit. And I should like to ask Mr. Christie the question, whether when dealing with these grazing incidences he does not run the risk of losing definition from slight errors in the working of the surfaces of the prisms?

Lord Lindsay: There is one thing with regard to Mr. Christie's statement I should like to comment upon. He says that with large prisms you get less light than with smaller ones. This, I think, is contrary to my experience. With my large prisms I have had to make use of a dark glass in looking at the spectrum of the sun through seven prisms; the height of the prisms is 3·6 inches, with a base of about 5 inches.

Mr. Maclean: It seems to me that very perfect adjustment will be required to get the whole of the field of view into focus at one time.

Mr. Gill: I think Mr. Christie attaches great importance to the absorption of light. Of course, the thicker the glass, the more light is lost; but there are many advantages connected with large prisms, and in his form of spectroscope it must not be lost sight of. If the slit is magnified in width as it is, then the proper way to get a manageable slit of small angular aperture is to have a long collimator. If you have a long collimator, you must have a large aperture, and if you have a large collimator, you lose light if you have not a prism large enough to take in the whole of the rays. Therefore, to utilize Mr. Christie's prism in the highest form, you must have a considerable aperture to do justice to it. And as to the enormous absorption which Mr. Christie has found, surely he must have been making use of prisms of very bad glass. I have seen through Lord Lindsay's big spectroscope, and made a great many measures with it, and if there had been only 1-150th part of the light, I do not think I could have seen so far to the violet end of the solar spectrum as I have done. I have seen through so many prisms too well-defined spectra of the stars. I have not had the advantage of making accurate photometrical determinations, but 1-150th part of the light would be a small residue indeed.

Mr. Christie: I did not see the spectrum of Sirius through ten prisms, and nobody could see it, I should imagine.

Mr. Gill: I have seen Sirius through four of Grubb's compound prisms when the ray has been sent forward and backward.

Mr. Christie: That makes a difference.

Mr. Gill: I think that if there were only 1-150th part of the light of the spectrum that you would not see it at all.

Mr. Christie: I have tried various specimens of glass, and I found the glass I tried first in our prisms is decidedly more transparent than that I have come across since except some very dense flint. I measured it with my polarizing photometer eyepiece, and I had a check on the intensity of the light passing through the prisms, by comparing it with that reflected from the front surface of the first half-prism, and I found that through about eleven inches of glass the light was reduced to about 1-16th or 1-20th part. We have, I think, about thirty-five inches of glass in the whole train of prisms. The absorption increases as an exponential function of the thickness, and therefore it increases very rapidly. The general result I arrived at was that, with three or four inches of glass, the absorption would reduce the light to one-half, and the next three would reduce it to one-fourth, and the next three to one-eighth. So that when you come to thirty inches, you will see the light would be reduced enormously.

Mr. Gill: I must say, speaking for myself, it seems a staggering fact. I do not, however, throw the slightest doubt on what Mr. Christie has said, because he has measured and I have not.

Mr. Christie: I quite agree with you in that. I agree also generally with the remarks that have been made. I do not object to any criticism, but I fear I have not made myself clearly understood. It is an intricate subject to deal with, and much more so than I bargained for. I think Mr. Bidder pointed out very fairly that the great dispersion was accompanied by the broadening of the lines. That was what I mentioned. That is the case, and was one of the things that struck me, but when I came to think about it, I saw that you do not gain anything in these matters, except at the expense of something else. Of course, you can narrow the slit, and you can get the lines narrower, but this is at the expense of light. So much light is absorbed in the ordinary isosceles prism that you cannot use a narrow slit. In this you can use a narrow slit, and get greater purity; but by reversing the instrument, you can also get greater purity, but less length of spectrum. You can make use of the instrument for two classes of spectroscopic work. In the one case, using the instrument directly, you get a great separation of the lines, and with this you can measure the position of the lines very accurately, the test being that you are able to apply a high magnifying power; and in the other case, when you want great purity (for example, in viewing the solar prominences), and light is of little consequence, you can use the instrument reversed. Lord Lindsay spoke about the importance of having a large aperture. As to this I also fully agree, though not with respect to the absorption. The importance of the aperture is certainly very great. I tried the experiment of sticking two prisms together side by side, so as to get greater aperture. There are two objections to increasing the aperture by simply increasing the prisms—first, you get greater absorption through the increased thickness of the glass; and secondly, it is very difficult to get glass homogeneous. In putting two prisms side by side, I found the definition quite as good as with one. No doubt it is of great importance to have a large aperture, and that is the direction in which, I imagine, spectroscopes may be improved, because as you increase the magnifying power, you magnify the diffraction image, which depends, as Mr. Gill says, on the length of the collimator. With reference to the defects of the surface, which Mr. Bidder referred to, I did not expect I should have been able to use surfaces with such oblique incidence. It is true that as you increase the obliquity you increase the divergence of the rays, but in the ordinary form of spectroscope, whatever error may be in the surface gets repeated, and mounts up, whereas if you use two prisms only instead of ten, you have more chance of getting two good ones than ten good ones, besides which the first prism may be a larger one, and the other much narrower. I am afraid I cannot give a full explanation of this, because there are so many points connected with it which one is apt to overlook. This is the best explanation I can give at present.

Mr. Ranyard: With regard to the increase in the size of the prisms being more than counterbalanced by the absorption, if this is the case, it will follow that, if the course of the ray through prisms of a certain size is fifteen inches, and if you double the diameter of the prisms, and at the same time double the diameter of the instrument and collimator so as to get a pencil of double the diameter, or four times as much light entering the prisms, still the absorption of the second fifteen inches; for the course of the ray through the glass will only be doubled by doubling the prisms. The absorption of the latter half of the prisms will more than counterbalance the quadruple area of the prisms. This question is a very important one. I certainly should not have expected to find the law as Mr. Christie states it. I should have expected to find that the rays which had got through the first half would suffer much less absorption in the second half.

Mr. Christie: That is, however, not the case, for the light transmitted follows an exponential law. It goes by the inverse square, cube, and so on, so that it will decrease very rapidly after four inches.

The President: I will ask you to return our best thanks to Mr. Christie. It is quite obvious to any one who has worked at the spectroscope, that it is impossible in one instrument to have all the things one desires. You have to select and balance, and possibly the spectroscope of the future will be formed upon a balance of advantages and disadvantages. What an immense advantage it would be to obtain a large dispersion equal to that which has been, up to the present time, obtained by a large number of prisms without the consequent loss of light, I am sure many others, with myself, have been waiting with interest to see what we shall see when this other spectroscope of Mr. Christie's has been completed.

Professor Pritchard said that for a long time no comets had been observed, because, he supposed, no one had looked for them. D'Arrest's comet had been looked for in vain, but two other comets had been found. On the 20th of February Borrelly's comet had been observed at Oxford, and he was glad to report that, owing to a good deal of trouble which he had taken, and to the energy and diligent application of his assistants, Mr. Plummer and Mr. Jenkins, they had observed the comet fourteen times since. On April the 4th the comet was too faint to be seen, and now, probably, it has been lost sight of; though D'Arrest's comet had not been seen, it is still possible that it may be caught sight of in August.

Professor Pritchard had received a telegram from the Astronomer-Royal announcing the discovery of a comet by Winnecke, and they had immediately proceeded to search for it. They caught it the same night, at Oxford, and again on the 7th and 11th, and from these three observations they had calculated an orbit which, he thought, ought to be the best which had been at present published, because founded on observations taken at wider intervals of time.

Capt. Noble protested against the comet being called "Winnecke's comet," as that name was now universally recognized by astronomers as belonging to the well-known comet of about five and a half year's period, which is not due until the autumn of 1880.

Professor Pritchard said that he had not simply called it Winnecke's comet, but had referred to it in his paper as comet 3, 1877.

Lord Lindsay said that he had yesterday received a letter from Dr. Copeland mentioning that he had seen the comet at Dun Echt, and that it had a tail 18′ long. He had observed it with a spectroscope, and had found three bright lines with a continuous spectrum from the nucleus. He thought that the comet would be very shortly visible to the naked eye.

Professor Pritchard read another paper, entitled, *On two mechanical solutions of Kepler's problem.* He showed his instruments, and worked an example on the black board. He thought that his mechanical method compared very favourably in point of rapidity with the computational method proposed by Signor de Gasparis in his paper published in the last number of the monthly notices.

Mr. J. W. L. Glaisher read and explained upon the black board a method of solving Kepler's problem by the use of elliptic functions.

Father Perry was called upon to read a paper on his unsuccessful search for the planet Vulcan, he said: We made use of our large instrument of 8 inches aperture equatorially mounted, and projected the sun on a screen, and got beautiful definition. Three of my assistants were watching, but no planet was seen, though there were some beautiful sun-spots.

I have also the honour of presenting two papers by M. A. Angot, who was sent by the French Government as companion to M. C. André, to observe the transit of Venus at Noumla. Since their return they have been studying in preparation for 1882. The French government are going to send them out to the transit of Mercury next year. M. Angot has got some photographs which show this curious result, that the photographic image is in some of them larger than the geometric image, and in some smaller. This cannot be explained by the ordinary theory of irradiation, but it is fully explained by the theory of diffraction started by M. André. The paper will, I think, be read with interest.

Mr. Ranyard read extracts from a paper by Mr. Rand-Capron, entitled, *Report of examination of the sun's disc, at Guildown, Guildford, on 21st, 22nd, and 23rd March,* 1877, *for the suspected planet Vulcan.* Nothing like a planet had been seen upon the sun's disc. Mr. Rand-Capron had rendered his negative evidence permanent by taking photographs of the sun's disc with an ordinary camera box placed so as to receive the image from the telescope. In order to give the short exposures which were necessary he had contrived screens of zinc with slits in them, which were allowed to fall through the camera by their own weight.

The following papers were also announced and partly read:—

Sur l'application de la photographie à l'observation du Passage de Vénus: A. Angot.

Etudes sur les images photographiques obtenues au foyer des lunettes astronomiques: A. Angot.

Radiant points of shooting stars from Captain Tupman's unreduced observations 1869—71: W. F. Denning.

Visible transits of Mercury to the year 2000: Rev. S. J. Johnson.

Nebuleuses nouvelles découvertes et observées à l'Observatoire de Marseille: E. Stephan.

On personal equation: Capt. W. M. Campbell.

The great meteor of 1877, *March* 17: Capt. G. L. Tupman.

Observations of occultations of stars by the moon, 1875—6—7, (*with the deduced equations between the error of the lunar elements*) *and phenomena of Jupiter's satellites, made at the Radcliffe Observatory, Oxford*: Rev. R. Main.

On the inequality of the moon's longitude discovered by Professor Newcomb: E. Neison.

Ephemeris for physical observations of Mars, 1877: A. Marth.

Extract from a letter describing an ancient Arabic celestial globe: Prof Meucci.

The sidereal system: Maxwell Hall.

Description of an improved diagram for the graphical solution of spherical triangles: F. C. Penrose.

Heliometer observations of the planet Juno: Lord Lindsay and Mr. Gill.

Nebuleuses nouvelles decouvertes et observées a l'Observatoire de Marseille: E. Stephan.

Comet II. 1877 *(Extract of a letter to Mr. Hind)*: Professor Winnecke.

The meeting adjourned at ten minutes past ten till Friday, May the 11th.

REVIEWS.

Dun Echt Observatory publications. Vol. I. *A Summary or Index of the measurements in the "Stellarum duplicium et multiplicium mensuræ micrometricæ."* F. G. W. Struve, 1837; "*Additamentum in F. G. W. Struve mensuras micrometricas stellarum duplicium,*" *editas anno* 1837. *Petrop.*, 1840. *Including all the stars in the* "*Synopsis Observationum de Stellis duplicibus, in Specula Dorpatensi, annis a* 1814 *ad* 1824, *per Instrumenta minora perfectorum,*" p. 305; *and in the* "*II. Mensuræ Micrometricæ,*" p. 315. *Re-arranged in order of R.A., and the positions brought up to* 1875. Dun Echt, Aberdeen, 1876 (pp. 129).

We transcribe the preface. "In sending forth the following summary of the 'Mensuræ Micrometricæ' of the illustrious Struve, I have been actuated by the feeling that such a work might prove welcome, and be of some service to my fellow worshippers at the shrine of Urania. The scarcity of the original publication, added to the size of the volume itself, has caused this invaluable catalogue to be found in fewer hands than its great merit deserves. Public observatories, worthy of the name, of course, possess it; but in the more humble temple of the amateur astronomer it is usually in the list of desiderata. Under these circumstances, and considering also that the epoch of the Catalogue is already remote, I am emboldened to hope that this summary of the Catalogue, as re-arranged in order of R. A., by Mr. R. Copeland, brought up to the date of 1875, and presented in a portable form, may serve to supply the wants of those who take interest in Double Star Work. The thanks of users of the Catalogue are due to this gentleman for the care and patience he has bestowed on the reductions, etc. The description of the arrangement of the Catalogue which follows will need no further explanation, save that the translation of the Latin names of the colours has been carefully considered and rigidly adhered to.—October, 1876. LINDSAY."

Lord Lindsay has indeed rendered a great service to astronomers, especially amateurs, by the issue of this very valuable Summary of Struve's observations of double and multiple stars. Their number is 3,062; on each page we have Struve's number, and reference to the pages of his work; the star's name (if it has one); its R. A. and Decl. for 1875; the magnifying power used; date of the observation; distance and positions of the components; and their magnitudes and colours; of colours there are forty-two designations. Appended is a table of the precessions in declination for *ten years*, and a diagram from which the variation in precession in R. A. for *ten years* can be taken by inspection.

All who use this catalogue will wish for Lord Lindsay and Mr. Copeland that (in Oriental parlance) their shadows may never be less!

A Handbook of Descriptive Astronomy. By George F. Chambers, F.R.A.S., of the Inner Temple, Barrister-at-Law, &c. Third edition. Oxford: at the Clarendon Press. 1877. (pp. 928. 28s.)

We congratulate Mr. Chambers that amidst the demands of his professional work he has been able thoroughly to revise his well-known and highly appreciated "Descriptive Astronomy," so that it may not be left behind by the astonishing development which, as he remarks, has taken place in the science during the last ten years. We can testify to the truth of his declaration that scarcely a single page has not been amended, and the amount of new matter is not less than 200 pages. He has also had the advantage of a great amount of very useful advice and assistance from observers in all parts of the world, some of whom are mentioned by name.

This thick and handsome volume represents much thought and trouble, and we cordially wish life and health for its author to enable him to keep pace with the advance of astronomy during another ten years, which we may reasonably expect will not be inconsiderable. Amongst the many new illustrations are Coggia's comet, 1874, July 13th (Brodie); drawings of the great sun-spot of Oct. 11th, 1865 (Brodie); diagram illustrating the connection between auroræ, terrestrial magnetism, and spots on the sun (Loomis); Jupiter, 1871, Dec. 30th (Lassell); eclipse of the sun, 1874 (Bright); Encke's comet, 1871 (Key and Carpenter); the nebula κ Crucis (Russell, Sydney, N.S.W., 1872) and a large number of reflectors with various mountings. The number of illustrations in the present edition is 286, against 224 in the preceding.

In the preface we are told that "the most important changes will be found in the chapters dealing with the sun, sidereal astronomy, and astronomical instruments. The descriptions of clusters and nebulæ have been made more numerous, and the list of objects critically revised one by one actually at the telescope, so as to make that portion of the work more completely than formerly a *vade mecum* for the mere star gazer who is an astronomer simply in the respect that he is the owner of a telescope. Indeed, it has been chiefly with this idea in view that so much additional matter has been introduced into the chapters relating to astronomical instruments. The 'Practical Hints' and suggestions have been gathered from so many sources, and embody the collective wisdom and experience of so many men, that they cannot fail to deserve attention. I believe also that this volume now stands alone in its full description, so far as regards the wants of amateur observers, of the mounting and use of reflecting telescopes."

The lists of binary and new stars will be very acceptable. Book X. on spectroscopic astronomy is written by Mr. B. W. Maunder, of the Spectroscopic Department of the Royal Observatory. Chapter VII. on celestial photography is by Mr. A. Brothers, F.R A.S., of Manchester. Both are valuable. In the revision of the list of recorded comets the late Mr. J. Williams' catalogue (1871) of Chinese comet observations has been much used. Book VII. on practical astronomy has been greatly improved and enlarged, and the same is true of Book IX. on meteoric astronomy. Those interested in the minor planets, of which we have rather an *embarras de richesses*, will find here the components of the whole zone from Ceres to Zelia (discovered 28th Sept. last) No. 169. But the list is already incomplete through the subsequent discovery of several new ones, and very likely we are not nearly come to the end of them. Mr. Chambers says several of them have been lost, such as Sylvia, Dike and Camilla. Others, as Lydia, have been found again after being lost. We agree with Mr. Chambers that many names given to these little planets of late years are most fantastic and ridiculous. It does not say much for the taste of these "earthly godfathers of heaven's lights," as astronomers are called in Shakespeare.

Of course more detail may be found in books devoted to special departments of the science, but as a compendium of the *tout ensemble* of descriptive astronomy we believe this work is unrivalled in the English language. At the end of the preface to the second edition (1867) Mr. Chambers expressed a hope that his book might be "the means of inducing some at least to interest themselves in the study of that noble, but by no means abstruse science which in so conclusive a manner shows forth the wonderful wisdom, power, and beneficence of the Great Creator and Omnipotent Ruler of the Universe." In all probability this hope has been and will yet more be realised. For ourselves, there is hardly a work we have more frequently turned to for information, without either finding or being directed where to find it. There may yet be in it some mistakes and omissions. It is natural that the former should be repeated in various books, and sometimes remain a long time either not detected or else not publicly corrected. Thus, on p. 6 of this work, near the bottom, 60 miles should be *feet* (or is it 66 feet, as given in Sir Edmund Beckett's "Astronomy without Mathematics," p. 103, 5th edition?) On pp. 537, 538, a line or two might have been well added to explain to the reader how it is that the view of the dumb-bell nebula with Lord Rosse's 3-ft. reflector is so different from what is seen with the 6-ft. reflector, the *former* showing so much more nebulosity. This, which is owing to different magnifying powers (high powers extinguishing the nebulosity) is explained by Lardner (Dunkin's edition, pp. 436. 437). In a note p. 848, Mr. Chambers speaks of the arbitrary and unphilosophical metric system of the French. We thoroughly coincide with this. It is an abomination, and mars our enjoyment of every French scientific work we read. We do not see in the cat. of celestial charts mention of Argelander's splendid series of star charts, 40 in number, containing upwards of 320,000 stars,* (which Mr. Proctor so marvellously compressed into a 2-ft. circle). In conclusion we need hardly say that in every respect this work is turned out in a style worthy of the Clarendon Press.

LES TERRES DU CIEL. *Description Astronomique, Physique, Climatologique, Geographique des Planètes qui gravitent avec la Terre autour du Soleil, et de l'état probable de la vie à la surface.* Par Camille Flammarion. Paris, Didier et Cie. pp. 600.

We have much enjoyed the perusal of this brilliant and very interesting work, though by no means agreeing with some of the author's reflections. Such a collection of facts about the "great solar republic" we have not before met with, and the illustrations are copious and excellent. The following are M. Flammarion's views on some disputed points. The *lumière cendrée in Venus,* he thinks is due to a diffuse reflection produced by clouds. "The very white globe of Venus reflects vaguely the stellar light scattered through space, whilst space itself remains absolutely dark. It is enough that this brightness be faintly developed, for the eye to continue instinctively the contour of the crescent, and guess at the rest of the globe which otherwise it would not distinguish. We shall see further on that this world is almost always surrounded by clouds of singular whiteness. Perhaps they may have a kind of phosphorescence such as our own sometimes display in spring. Perhaps, also, we may behold auroras in the atmosphere of Venus. The white clouds . . . their possible phosphorescence, or auroras, are sufficient to explain the lumière cendrée." p. 189. *Lunar mountains with rays.* "Of what nature are these radiating bands? After having long and attentively observed them with the aid of instruments of all powers, I have been led to think that they represent cracks in the lunar globe which has yielded to a strong internal pressure, principally around the foci of the more important craters; not cracks which have been filled with lava from the interior, as Nasmyth supposes, but merely lines which have served for the passage of heat and gases that have vitrified and whitened the ground in their course. There remain afterwards simply traces, a mere outline of the phenomenon. It is an operation rather chemical than mechanical. These fissures do not form crevasses, for they are not hollow, nor ridges, for they are not in relief." p. 334. *Projection of Stars on the Moon's disc.* "The best explanation is that which ascribes it to an atmosphere existing especially in the hemisphere which we do not see, and which from time to time is brought by libration towards the edge of the Moon." pp. 355, 356.

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* We may observe here that a new edition (the 3rd, 1877), has just appeared of the *Atlas Celeste* of Ch. Dien, corrected and enlarged by M. Flammarion. It now consists of 31 plates on copper, five being double. Its price with paper cover, is 40fr., and in cloth boards, 45fr. The 5 new charts (Nos. 25 to 29) by M. Flammarion, relate to the principal objects of Astronomical studies, with letter-press explanatory, and is sold also separately, 15fr. Paris, Gauthier-Villars.

And about a *lunar atmosphere:* "To sum up then, there may (and there should) be on the moon an atmosphere of feeble density, and of a composition probably very different from ours. Perhaps there exist also certain liquids, as water, but in a very small quantity. If there were no air at all, a single drop of water could not subsist there, since it is only the atmospheric pressure which keeps water in the liquid state, and without it all water would immediately evaporate. Lastly, it is possible that the lunar hemisphere we never see may be more rich in fluids than this one. But it is plain in every case that it would be opposed to the candid interpretation of facts to affirm, as is too often done, that there is absolutely no atmosphere, nor any liquid or fluid on the surface of the moon." p. 360.

On the *habitability of the Moon*, M. Flammarion says: "There is nevertheless a high probability that lunar life is more advanced than that of the earth, and is actually in decadence. Activity is no more there what it once was. This world is certainly in a state of calm and repose of which no other world of our system presents an example. This is an incontestable fact. But this calm which environs the lunar world, and of which we have so deep an impression when we observe with the telescope her motionless tracts illumined in the night, if it be a witness of relative repose in that earth of old so agitated, is still not a witness of its death. Doubtless the lunar life is in its decline, but probably it has not yet disappeared, and perhaps the last families of lunar humanity are still there at the bottom of the valleys, in the velvety plain of Plato, or in the wavy valley of Hyginus, or on the shores of the sea of Serenity, contemplating us from their abode, and asking themselves how a planet so agitated as ours, and also saturated with fogs, can be inhabited by delicate and intelligent beings." p. 372. The author is an ardent advocate of the existence of life almost everywhere in the universe, though its development, he contends, varies in every globe. He never loses sight of the words he has placed on the title page, "It is not solitude and death that astronomy shows us in the heavens: it is LIFE, universal and eternal."

M. Flammarion does not mention Mr. Lockyer's having succeeded in viewing the solar prominences with the spectroscope, as well as M. Janssen; and that the successes of both were simultaneously announced, p. 102. And he has misunderstood M. Hind ("Solar System," p. 33.) whom we do not apprehend to mean that the direction of the earth's axis was changed after the Flood, p. 274. On p. 277, he points out an error in Sir John Herschel's Astronomy, followed by Chambers,* in making the earth's perihelion coincide with the *Vernal* equinox in 4,000 B.C., whereas it then coincided with the autumnal equinox. He might have pointed out, however, that the same mistake is found in Laplace's *Système du Monde* (5th ed. p. 197). Whether or not the reader is prepared to go all lengths with M. Flammarion in his idea of the universality of life (and it is ably supported) he will find this volume one of great interest, and valuable as a work of reference.

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* Corrected in the 3rd ed. of his *Astronomy*.

CORRESPONDENCE.

N.B. —We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

OBSERVING NIGHTS IN 1876.

	Clear all night.	Very good.	Fair.	Bad.
January	1	—	3	2
February	1	—	0	3
March	2	—	3	2
April	5	—	3	5
May	6	1	6	5
June	5	—	4	2
July	3	—	4	4
August	6	—	3	5
September	2	—	3	0
October	3	—	3	6
November	3	—	1	3
December	1	—	1	3
	38	1	34	40

HALOS SEEN IN 1876.

	Solar halos.	Lunar halos.	Auroræ.	Lightning.
January	1	1	—	—
February	0	1	1	—
March	8	3	—	5
April	5	2	—	2
May	10	0	—	—
June	7	1	1	1
July	10	1	—	3
August	4	0	—	7
September	3	0	—	2
October	1	0	—	3
November	0	2	—	—
December	0	1	—	—
	49	12	2	23

Notes.—In May a horizontal circle was seen, its plane passing through the sun.

In June, a vertical beam was seen above the setting moon.

Mr. E. Crossley's Observatory, T. GLEDHILL.
Halifax: March 9, 1877.

ON THE NEW STAR IN CYGNUS.

Sir,—On the 13th, 17th and 20th of the present month, I examined Prof. Schmidt's new star in Cygnus, and found it about 8m., being about equal to or perhaps slightly less than Lalande 42383 (8m. Lalande, Harding) which lies about 24′ south of the Nova.

The star Lalande 42360 (7m.) closely preceding LL. (42383) seems to have disappeared, as with a powerful binocular, which shows stars to about 9½m., I fail to see any trace of it.

The star Lalande 42306 (about 23′ *s. p.* the place of the Nova) would also seem to have faded. It was rated 8m. by Lalande, and is marked 8m. in Harding's atlas, but with the binocular above referred to, I can only catch it by glimpses with difficulty, and it is probably not above 10m. at present.

Between the new star and L. L. 42383, a little *preceding* is a small star (about 9m.) which is not in Lalande's catalogue.

Umballa, Punjab: Yours faithfully,
Jan. 22, 1877. J. E. GORE.

ON A NEW VARIABLE (?) STAR IN CETUS.

Sir,—About 1¼° *n.p.* ν^2 Ceti (59 Flamsteed) Harding shows a 6m. star which is not in Lalande's Catalogue, although the position of an 8½m. star about 13″ is recorded (Lalande 3590). On the 18th of last month I found Harding 6m. star only 8m., being then brighter than L. 3590, but considerably fainter than a 7m. star, shown by Harding, a little *preceding* ν^2. As it is well known that Harding compared his charts with the heavens, this 6m. star may prove to be variable. The place of Lalande 3590 is for the beginning of the present year—R.A. 1h. 49m. 58s., and N.P.D. 111° 9′ 56″.

Umballa, Punjab: Yours faithfully,
Dec. 7, 1876. J. E. GORE.

IS α CETI A RADIANT POINT?

Sir,—Meteors have been recorded by me for the past twenty-one months, invariably from 9 to 9.30 p.m., each clear night in the absence of moonlight. The number seen on an ordinary night during a half hour's observation averages from 1 to 3; but occasionaly 4 or 5 are recorded, and more rarely 6, this being the extreme number recorded during the 21 months, and then only five times out of 152 observations, namely—May 26th, July 30th, and Oct. 31st, 75, and Oct. 17th and Nov. 13th, 76. When the number seen during a half-hour's observation reaches 4 and over we naturally look for a shower, as the yearly average from 9 to 10 p.m. is only 4.7, or 2.4 for half an hour. Of the number recorded on the exceptional nights mentioned above, when 6 were seen, their motions were generally quick, and the magnitudes small: but an exception was noticed of those recorded Oct. 17th, 1876, and for the number seen I never recorded so many brilliant ones, there being one of the first magnitude, three of the second, and two of the fourth. Of the six recorded, three radiated directly from α Ceti—namely, the 2, 3, and 4 of the second, first, and third magnitudes, the last two moving very slowly over a large space. The 18th was cloudy, but on the 19th and

20th, of four recorded, two also radiated from this point of the second and third magnitudes, and moving slower.

I remain, yours with respect,
36, School-street, Boston, Mass., U.S.: EDWIN F. SAWYER.
Boston, Feb. 12, 1877.

THE RING OF LINNE.

Sir,—I have been much surprised in reading in the January number of the *Quarterly Journal of Science*, a statement by E. Neison, that the large faint ring of Linné has not been seen since 1869, implying that is not now visible, or, at least, is very indistinct. But I saw it in 1873, 1874, 1875, and 1876, with a 4½-inch refractor, with which it is not a very difficult object. It is not visible for many hours at each time. I do not deny that, according to E. Neison's suggestion, it may really be only a series of ridges and mounds; but, with my telescope, it appears as a complete ring.

Sunderland: April 10, 1877. THOS. WM. BACKHOUSE.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN MAY, 1877.

By W. R. Birt, F.R.A.S., F.M.S.

The Mare Crisium eastern border. Schröter in his drawing of the Mare Crisium in Tafel VI. of his Fragments, gives three craters on and near the eastern mountain border south of the "Pass" and four mountains north of it. Of the three craters the two southern ones are *in a line* with Picard. The nearest to Picard is *f* described in the lunar catalogue as the ancient ring of Schröter I C λ^2, but shown by him as a crater. The farthest is μ on the mountain border I C λ 20. North of μ is the crater marked by him ν on Tafel VI., but β on fig. 2 of T. XXXV. In this figure he gives an additional crater α not in T. VI. These have been registered as I C λ 17 and I C λ 18. In T VI. I C λ 2 and I C λ 20 are *in a line* with Picard, but in fig. 2 T. XXXV. I C λ 2-17 and 18 are *in a line* and I C λ 20 is absent. As but few of the most favourable opportunities for viewing the eastern border of the mare are left—viz., May and June, it would be well if the μ of Schröter could be looked for, and, if possible, recovered. The four mountains north of the "Pass" are common to both drawings, and are marked *l, m, n, p; l* was seen and described several times by Schröter *as a crater*. It is I C π 8, and is situated on the northern extremity of the ridge I C λ 9. See *English Mechanic*, Jan. 1877, p. 425. The height of this crater on its exterior rim was measured by Schröter on the 28th of April, 1789, and found to be 4674 Parisian feet. The mountain *m* is registered in the lunar catalogue as I C π 11, and described as N. W. of I C π 8. It is between I C π 8 and I C π 1 (Peirce of Neison's map). Schröter's measures give 5418 Parisian feet for its height; *n* and *p* are *in a line* with *l*, both N.E. of it; the measure of *n* I C π 12 of the catalogues is 10,676 Parisian feet; *p* I C π 13 was not measured at the same time as *l, m,* and *n*. Notices of the eight objects as seen during any portions of the lunations of May and June are desirable. Between June and December their appearance under the evening illumination may be well observed.

Hankenbury, Buckhurst Hill:
April 24, 1877.

ASTRONOMICAL OCCURRENCES FOR MAY, 1877.

DATE.			Principal Occurrences.	Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Spica
Tues	1	12	Conjunction of Moon and Jupiter 4° 59′ N.			10 38·9
Wed	2		Sidereal Time at Mean Noon, 2h. 42m. 0·92s.			10 34·9
Thur	3		Sun's Meridian Passage 3m. 17·22s. before Mean Noon	1st Sh. I.	16 5	10 31·0
Fri	4	23 18 14 37 15 1 15	☾ Moon's Last Quarter Occultation of η Capricorni (5½) Reappearance of ditto Conjunction of Moon and Mars 0° 39′ N.	1st Ec. D.	13 23 57	10 27·1
Sat	5			1st Tr. I. 1st Sh. E. 1st Tr. E. 1st Sh. I. 2nd Tr. I.	11 31 12 49 13 46 14 0 15 53	10 23·2
Sun	6	13	Superior conjunction of Venus and Sun			10 19·2
Mon	7	20	Conjunction of Moon and Saturn, 3° 3′ S.	2nd Oc. R. 3rd Sh. I.	13 0 16 2	10 15·3
Tues	8					10 11·4
Wed	9					10 7·4
Thur	10	19	Uranus at quadrature with Sun Saturn's Ring: Major axis=37″·06 Minor axis=0″·94			10 3·5
Fri	11			3rd Oc. D. 1st Ec. D.	12 16 15 17 49	9 59·6
Sat	12	12 23	● New Moon Conjunction of Moon and Venus 5° 32′ S.	1st Sh. I. 1st Tr. I. 1st Sh. E. 1st Tr. E	12 26 13 17 14 42 15 32	9 55·7
Sun	13			1st Oc. R.	12 49	9 51·7
Mon	14	1	Conjunction of Moon and Mercury 3° 43′ S.	2nd Ec. D. 2nd Oc. R.	10 59 54 15 20	9 47·8
Tues	15		Illuminated portion of disc of Venus=0·999 Illuminated portion of disc of Mars=0·860			9 43·9

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Spica
Wed	16		Sidereal Time at Mean Noon 3h. 37m. 12·71s.			9 39·9
Thur	17		Sun's Meridian Passage 3m. 48·91s. before Mean Noon			9 36·0
Fri	18			**3**rd Ec. R. **3**rd Oc. D. **3**rd Oc. R.	12 39 32 13 1 15 43	9 32·1
Sat	19	12 56	☽ Moon's First Quarter	**1**st Sh. I. **1**st Tr. I.	14 20 15 2	9 28·1
Sun	20			**1**st Ec. D. **1**st Oc. R.	11 40 16 14 35	9 24·2
Mon	21			**1**st Sh. E. **1**st Tr. E. **2**nd Ec. D.	11 4 11 44 13 33 39	9 20·3
Tues	22	12 39 12 43	Near approach of χ Virginis (5) Near approach of B.A.C. 4259 (6)			9 16·3
Wed	23	11	Conjunction of Venus and Mercury 1° 3′ S.	**2**nd Sh. E. **2**nd Tr. E.	11 16 12 32	9 12·4
Thur	24					9 8·5
Fri	25			**3**rd Ec. D.	14 7 19	9 4·5
Sat	26	16 5 8	○ Full Moon Inferior conjunction of Mercury and Sun			9 0·6
Sun	27			**1**st Ec. D.	13 34 18	8 56·7
Mon	28	16	Conjunction of Moon and Jupiter 4° 51′ N.	**1**st Sh. I. **1**st Tr. I. **1**st Sh. E. **1**st Tr. E.	10 42 11 13 12 57 13 29	8 52·7
Tues	29	15 30 15 51	Occultation of τ Sagittarii (3½) Reappearance of ditto	**1**st Oc. R.	10 47	8 48·8
Wed	30		Saturn's Ring : Major axis=38″·24 Minor axis=0″·62	**2**nd Sh. I. **2**nd Tr. I. **2**nd Sh. E. **2**nd Tr. E.	11 6 12 3 13 53 14 50	8 44·9
Thur	31	12 55 14 13	Occultation of 17 Capricorni (6) Reappearance of ditto			8 40·9
JUNE Fri	1					8 37·0

THE PLANETS FOR MAY.

At Transit over the Meridian of Greenwich.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	3 57 26	N.23 9	7″·6	1 19·1
	9th	4 23 13	N.23 55	9″·2	1 15·4
	17th	4 31 6	N.22 36	11″·0	0 49·8
	25th	4 18 60	N.19 52	12″·0	0 6·3
Venus ...	1st	2 35 8	N.14 19	9″·6	23 53·1
	9th	3 9 10	N.17 3½	9″·6	* *
	17th	3 49 10	N.19 45	9″·6	0 8·0
	25th	4 30 21	N.21 52½	9″·6	0 17·6
Mars ...	1st	20 48 58	S. 19 29	10″·9	18 7·9
	9th	21 8 57	S. 18 23	11″·7	17 56·4
	17th	21 28 14	S. 17 13½	12″·5	17 44·2
	25th	21 46 44	S. 16 2	13″·3	17 31·1
Jupiter ...	1st	18 14 45	S. 23 2½	40″·4	15 34·1
	9th	18 13 3	S 23 3½	41″·2	15 1·0
	17th	18 10 33	S. 23 5	42″·0	14 27·0
	25th	18 7 22	S. 23 6	42″·4	13 52·4
Saturn ...	10th	23 19 49	S. 6 19	14″·8	20 3·0
	20th	23 22 29	S. 6 4½	15″·0	19 26·3
	26th	23 23 51	S. 5 57½	15″·2	19 4·0
Uranus ...	1st	9 32 55	N.15 20	4″·2	6 53·7
	17th	9 33 34	N.15 16	4″·2	5 51·4

Mercury is very favourably situated for observation at the beginning of the month, setting more than two hours after the sun, the interval gradually decreasing.

Venus is too close to the sun to be well observed.

Mars rises about two hours after midnight at the beginning of the month, the interval decreasing to about half an hour after midnight on the last day.

Jupiter rises about half an hour before midnight on the 1st, the interval decreasing.

Saturn may be seen in the morning, rising on the 1st about an hour and a half before the sun. On the last day he rises one hour and a quarter after midnight.

THE MINOR PLANET JUNO.

The Minor Planet Juno, the third of the series, will come into opposition on the 31st of the month.

	Right Ascension.	Declination.
	h. m. s.	° ′ ″
May 1	17 7 22	S.5 47 24
„ 15	16 58 37	S.4 44 33
„ 31	16 45 47	S.3 52 42

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF JUPITER.

Greenwich Noon. 1877.	Angle of position of ♃'s axis. °	Longitude of ♃'s central meridian. °	diff.	Latitude of Earth above ♃'s equator. °	Equatorial diameter. "	Defect of illumination. "
May 1	357·53	95·3	870·7	—2·29	42·57	0·24
2	357·55	246·0	·7		42·69	·24
3	357·57	36·7	·7		42·81	·23
4	357·59	187·4	·7		42·93	·23
5	357·61	338·1	·7	—2·28	43·04	·22
—						
6	357·63	128·8	870·7		43·16	0·21
7	357·65	279·5	·7		43·27	·21
8	357·68	70·2	·7		43·38	·20
9	357·71	220·9	·8		43·49	·20
10	357·74	11·7	·7	—2·28	43·60	·19
11	357·77	162·4	·7		43·71	·18
12	357·80	313·1	·7		43·82	·18
—						
13	357·83	103·8	870·7		43·92	0·17
14	357·86	254·5	·8		44·03	·16
15	557·90	45·3	·7	—2·28	44·13	·16
16	357·94	196·0	·7		44·23	·15
17	357·97	346·7	·8		44·32	·14
18	358·01	137·5	·7		44·42	·14
19	358·05	288·2	·7		44·51	·13
—						
20	358·09	78·9	870·7	—2·28	44·60	0·12
21	351·13	229·6	·8		44·69	·11
22	358·18	20·4	·7		44·78	·11
23	358·22	171·1	·8		44·87	·10
24	358·26	321·9	·7		44·95	·10
25	358·31	112·6	·7	—2·28	45.03	·09
26	358·36	263·3	·8		45·11	·08
—						
27	358·40	54·1	870·7		45·18	0·08
28	358·45	204·8	·7		45·26	·07
29	358·50	355·5	·8		45·33	·07
30	358·55	146·3	·7	—2·28	45·40	·06
31	358·60	297·0	·8		45·46	·06
June 1	358·66	87·8			45·52	·05

The defect of illumination is on the preceding limb.

To find the longitude of ♃'s central meridian for

h.	°		h.	°	
8 add	290·2	to the longitude at	12 add	75·4	to the longitude at
9	326·5	noon.	13	111·6	noon.
10	2·8		14	147·9	
11	39·1		15	184·2	
12	75·4		16	220·5	

A. M.

Books received.—"The Window Observatory." By H. King, Capt. R.N. London: Crossley, Lockwood, and Co. 1877. "Superstition and reasonable belief." By Kuklos. London: John Harris, Kilburn Square. 1877.

THE NEW COMET.

We have received the following ephemeris of the new comet from the Secretary of the Imperial Academy of Sciences at Vienna :

T = 1877 April 28·2017

$\pi = 106^\circ \; 0' \; 12''$

$\Omega = 346 \; 59 \; 17$

$i = 77 \; 16 \; 1$

log. $q = 9{\cdot}99846$

$\delta \lambda$ cos. $\beta = -10$s.

$\delta \beta = -23$

Ephemeris for 12h. Berlin time.

1877	α h.	m.	s.	δ °	′	log Δ	log *r*	Lichtstärke
April 14	1	6	50	+55	8·0	0·1555	0·0103	1·00
18	1	42	4	57	54·5	0·1411	0·0045	1·10
22	2	25	6	60	5·2	0·1287	0·0006	1·18
26	3	15	20	61	19·1	0·1189	9·9986	1·25
30	4	9	33	61	17·8	0·1123	9·9988	1·29
May 4	5	2	34	59	54·8	0·1094	0·0010	1·29
8	5	49	50	57	18·9	6·1103	0·0052	1·26
12	6	29	24	53	49·1	0·1150	0·0113	1·20
16	7	1	30	49	46·5	0·1233	0·0190	1·11
20	7	27	23	+45	28·6	0·1346	0·0282	1·01

The elements show a very great resemblance to those of the comet of the year 1762.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Dec., 1876.

Horne & Thornthwaite, Messrs.

To March, 1877.

Homer, Rev. J.
Jackson-Gwilt, Mrs. H.

To June, 1877.

Squire, H.

To Dec. 1877.

Bates, T.
Bernaerts, G.
Birt, W. R.
Dancer, J. B.
Davies, Rev. R. P.
Guyon, G.
Hunt, G.
Ledger, Rev. E.
Lee, J.
Lewis, R. T.
Monk, Dr.
Worthington, A. M.

TO CORRESPONDENTS.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

When subscriptions sent by post are not acknowledged in the next number, the Editor will be much obliged if subscribers will *at once* inform him of the fact.

All Letters requiring an answer must enclose a penny stamp.

The Editor will be obliged if those gentlemen who have not paid their subscriptions will kindly send them by Cheque, Post-office Order, or *penny* postage stamps, but the Editor will not be liable for loss in transmission.

Post Office Orders for *the Editor* are to be made payable to JOHN C. JACKSON, at Lower Clapton, London, E.

The Astronomical Register is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings** per Quarter, ***payable in advance***, by postage stamps or otherwise.

The pages of the *Astronomical Registen* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. JACKSON, *Clarence Road, Clapton, E.*, **not later than the 15th of the Month.**

The Astronomical Register.

No. 174. JUNE. 1877.

ROYAL ASTRONOMICAL SOCIETY.

Session 1877—78.

Third Meeting of the Session, May 11th, 1877.

William Huggins, Esq., D.C.L., LL.D., &c., *President*, in the Chair.

Secretaries—Mr. Glaisher and Mr. Ranyard.

The minutes of the preceding meeting were read and confirmed.

William Maurice Beaufort, Esq., of 18, Piccadilly, and Athenæum Club,

was balloted for and duly elected a Fellow of the Society.

Mr. Glaisher said that 49 presents had been received since the last meeting of the Society, but none of them seemed to call for any special notice.

The President: In asking you to give your thanks to the donors of these presents, I must mention to the Society a fact which I am sure will be received with much pleasure, namely, that the Society has had a gift of £500. The late Mr. Lambert, of Upper Thames Street, left a large sum of money at the disposal of his son, to be given in part for scientific purposes, and his son has presented to this Society £500 free of legacy duty. The Council have this afternoon passed a special vote of thanks to Mr. Lambert for his very welcome gift, and I will ask you to return your thanks to him, as well as to the donors of these presents. (Hear, hear.)

The President: I have the unpleasant duty to give notice that two names will have to be suspended for arrears of payment. According to the Bye-laws, if a Fellow is more than three years in arrear, his name is to be suspended in the meeting-room. I believe in the case of these two Fellows that their addresses are not known. Every possible means has been taken to find their present addresses, but without success.

The Astronomer-Royal: Are the names to be read?

The President: The Bye-law only says the names shall be suspended, and I do not think I have any authority to read them. (Laughter.)

The President: I will now call upon the Astronomer-Royal.

The Astronomer-Royal: The matter I have to mention to you, I fear, hardly befits the gravity and dignity of the Society. My friend Captain Tupman, who is unable to attend, has asked me to mention to you a slight correction of a matter communicated by him to the Society at the last meeting, and recorded in the *Monthly Notices.* The character of it is rather singular. I will read it as it appears in the *Monthly Notices.* "A curious instance of the inaccuracy with which meteor tracks are sometimes described is that of J. N. W., in the *Times*, who says that 'in London the meteor was two-thirds the size of the moon when overhead.'" Upon that Captain Tupman remarks, "As a matter of fact it was never within 65° of the zenith of London." What I want to call attention to is this, that there has been a slight misreading of the communcation of J. N. W., and that the terms "when overhead" were applied to the elevation of the meteor, when they ought to have been applied to the size of the moon. It results from an inaccuracy and confusion in men's ideas, which probably may not have been known to my friend Captain Tupman. The description is: "In London the meteor was two-thirds of the size of the moon when overhead," that is to say, the meteor was two-thirds of the size of the moon as she would have been if she had been overhead. There is an odd idea amongst those who are not astronomers, that the moon does not appear so big when overhead as when on the horizon. Capt. Tupman says: "As a matter of fact, the meteor was never within 65° of the zenith," and of course nobody in his or her senses would describe an object so near to the horizon as that as overhead. But the observation merely had reference to what some observers have been accustomed to consider the size of the moon when overhead, which they think much smaller than when it is on the horizon. That this is a common idea I can support by the best of all authorities—poetry. (Laughter.) In the "Ancient Mariner" of Coleridge you will find he describes the state of things when the sun was vertical, as the unhappy ship passed through the equatorial regions. The lines run thus:

> "The sun stood up above our head
> No bigger than the moon."

I do not mean to say that Coleridge was perfectly correct in the whole of his astronomy. On one point at least he should not have said the sun was no bigger than the moon, because we know it is sometimes bigger and sometimes smaller, but his phrase conveys the idea that when the sun gets near the zenith he appears smaller.

The Astronomer-Royal then communicated a short paper containing extracts from letters relating to the supposed Intra-Mercurial planet. He said he had previously mentioned to the Society that telegraphic notices were sent to various distant stations requesting that a look-out might be kept for the supposed planet Vulcan. Most of the answers had been received by post, and they were all in the negative direction.

Mr. Penrose was called upon by the President to read a paper entitled *Description of an improved diagram for the graphical solution of spherical triangles.*

Mr. Penrose explained a diagram for determining by inspection altitudes from given hour angles and declinations, or reciprocally hour angles from given altitudes and declinations. The diagram was stated to be an improvement on that which he exhibited in Nov., 1876, for the purpose of giving a graphical determination of the corrections to be made in lunar observations for the spheroidal figure of the earth. The improved diagram which he exhibited was capable, not only of performing that operation, but of determining any zenith distance from a given polar distance and hour angle (excluding of course ill-conditioned spherical triangles) to about five minutes of arc or reciprocally as above stated. It consisted of a rectangular figure. On the top were given graduations of hour angles in equal spaces of five minutes each reckoned from left to right, that is, from the south meridian to the north, and polar distances measured downwards on the left hand vertical side from the same zero point, *i.e.*, from 0° of polar distance to 120°. The zenith distances corresponding to the hour angles and polar distances were marked for every 10° of zenith distance and joined by a continuous curve. This particular diagram was made for latitude 50°, but would be capable of interpolation for five degrees on each side of its own parallel of latitude. In using this diagram for latitude 50° it was only necessary to combine a perpendicular from the given hour angle with a horizontal line drawn through the proper polar distance, and these two lines would intersect at the zenith distance required. A scale on the diagram was given suited to every possible interval between the zenith distance lines, so as to enable the required distances to be read off at once.

Among the uses to which this diagram could be applied the case was mentioned of lunar observations at sea, when the moon's and the star's altitudes, as frequently happens owing to the haziness of the horizon, could not be properly observed, but when approximate altitudes would be sufficient for calculating the refraction. It offered also much facility in predicting occultations. For this purpose (as also for that of clearing the lunar distance when desired, by the graphical method which he had explained in his former paper) the angle at the moon, called the parallactic angle, would be required. This could be got from the latitude, polar distance, and hour angle, by the formula—Sine parallactic angle : sine hour angle : : sine colatitude : sine polar distance. Accordingly he had introduced at the bottom of the diagram a scale of logarithmic sines, similar to the scale given on the Slide rule, which enabled the parallactic angle to be obtained at once.

He then showed upon a diagram, suited to the prediction of occultations, how, for any assumed hour angle of the moon with respect to the star for which a prediction is desired, the parallax in altitude can be obtained in amount and in direction [using the previous diagram for obtaining by inspection the zenith distance and the parallactic angle], and that the point so found for the parallax in altitude when compared with the corresponding true place of the moon also laid down graphically, admits of being readily so adjusted that the distance between them (when an occultation is possible) becomes exactly equal to the moon's semi-diameter taken from a scale upon the diagram, and that the time and other circumstances required for the observation can be thus ascertained.

Mr. Gill said he had in his hand a chart prepared by Mr. Penrose for the island of Ascension, which would enable him to identify any star with which he was not familiar in the southern hemisphere. Mr. Penrose had also undertaken to predict for him by this graphical method the occultations which would be available, and he would thus be able very speedily to put Mr. Penrose's method to the test of practice. It appeared to him that one useful application of the method would be for finding the azimuth and altitude of stars near the moon. This would have been useful in the case of the transit of Venus expeditions, where latitude was determined by the altitude of the moon by the Astronomer-Royal's method.

Lord Lindsay read a paper *On the spectra of comets B and C, 1877, which had been observed at Dunecht.* Comet B (Borelli's) was examined on April the 11th. Its spectrum showed three nebulous lines, the least refrangible of which appeared to coincide with the position of the least refrangible band of the carbon spectrum. With a low power the spectrum of the nucleus appeared continuous. Comet C (Winnecke's) gave the ordinary carbon bands.

The President: This communication of Lord Lindsay's is of extreme interest to me because it is the first case that I have heard of which corroborates my observation of the spectrum of the small comet of Brorsen, figured by me in 1868. Some writers have evidently felt hesitation in accepting my observations of the spectrum of Brorsen's comet as really indicating a different position of the bands from the bands of the carbon spectrum, which are ordinarily observed. In Lord Lindsay's diagram of the spectrum of Borelli's comet, as well as in my drawing of the spectrum of Brorsen's comet, given in the *Philosophical Transactions* for 1868, there are three bands which might almost be called nebulous lines, for they are much narrower than the hazy bands of the cometary spectrum, which has been most frequently seen, and which has been identified with one of the forms of the spectrum of the carbon. The band towards the least refrangible part of the spectrum of Borelli's comet corresponds roughly with the brightest part of the band towards the least refrangible end of the carbon spectrum, but the other two nebulous lines distinctly differ in position from the two more refrangible bands of the carbon spectrum. Lord Lindsay's observation is one of extreme physical interest, as we have now definite confirmation of the fact that there are two distinct kinds of spectra presented by comets. The commonest one consists of shaded bands coincident with one of the forms of the spectrum of carbon, and the other, consisting of three bright lines or narrow bands, two of which occupy distinctly different positions in the spectrum from the bands of the carbon spectrum.

Capt. Noble mentioned that he had with his Ross equatorial of four and two-tenths inches aperture, and a small direct vision spectroscope, succeeded in seeing the gaseous spectrum of comet II. (he would not call it Winnecke's). He mentioned the fact as it might induce the possessors of small instruments to try the same experiment. It was encouraging to find that it did not require an 18-inch speculum to observe the gaseous spectra of these small comets.

In reply to the Astronomer-Royal, the President said he was not aware of the narrow line spectrum having been observed in more than the two comets which had been mentioned. The other form of spectrum was the same as that usually seen in comets.

Mr. Gill: It represents a carbon spectrum.

The President: Yes. I noticed a slight shift in the position of the lines in the spectrum of the bright comet which appeared in 1874. It presented quite the appearance of the carbon spectrum, and may be taken to be a carbon spectrum; but as to the other spectrum we do not at present know what it indicates.

Mr. De la Rue: The comet B seems to have lines near to the magnesium lines.

The President: Yes.

The thanks of the meeting were voted to Lord Lindsay for his paper.

The President: Lord Lindsay will now favour us with a few remarks on the very splendid instrument which is here, and which he has had the kindness to place at Mr. Gill's disposal for the observation of the coming opposition of Mars.

Lord Lindsay: I did not intend to trouble the meeting: I thought that it would be sufficient to show the instrument, because on several different occasions I have described the method of using the heliometer and also the method of its construction.

This instrument is the heliometer which was employed by Mr. Gill in connection with my expedition to Mauritius, and which I have lent to him for observing the approaching opposition of Mars at Ascension. The stand was made by Cooke of York, and does not differ in any marked way from the usual form of equatorial stand made by that firm, though the unusually low latitude of Ascension for which it is adjusted, gives it now an odd appearance. The heliometer part proper, including the cradle in which the tube rotates, was made by Repsolds, of Hamburg. The segments of the object-glass are mounted in cylindrical slides, the radius of whose curvature is equal to the focal length of the object-glass—thus the instrument is equally in focus whatever angle is measured, whereas in the case of Bessel's instrument, in which the slides were plains, the segments moved along tangents, and the distance from the eye-piece was as the secant of the angle measured. Another improvement is the mode by which motion is communicated to the segments. In Bessel's instrument this was effected by the action of a long screw, and in general one segment only was moved. In this instrument a very easy mode of communicating equal and opposite motion to the two segments is provided, so that all observations are made symmetrically with respect to the normal line of collimation. [Drawings showing the interior of the "head" of the instrument were handed round.] The separation of the segments is measured by silver scales attached to the slides which carry the two segments of the object-glass. These scales are so arranged that they move edge to edge and so can both be read at the eye-end by the same microscope passing down the inside of the tube.

This also is a great improvement on Bessel's instrument where screws were employed—the division lines of a scale once determined may be considered constant, but a screw is subject to wear. There are also great advantages in being able to read off the scales from the eye-end instead of having either to mount a perilously high ladder or turn down the end of the instrument between each observation.

In measuring position angles the whole tube, together with microscope handles, &c. is rotated, the amount of rotation being read off by a divided circle attached to the tube, referred to two verniers attached to the cradle. Two reading telescopes passing down to the eye-end enable these observations to be made with great facility.

The mode of rotation of the tube in the cradle, though greatly superior to that of the Oxford heliometer, seems still capable of improvement so far as slow motion in position angle is concerned, and I contemplate making some such improvement.

I should here mention that very many of the improvements now described were originally proposed by the elder Struve, though hitherto they have only been partially carried out.

The Astronomer-Royal asked whether any modifications had been made since the original construction of the instrument by Bessel.

Lord Lindsay said: Yes, a great many; there were cylindrical bearings of the two halves of the object-glass which were first spoken of by Struve; in fact nearly all the improvements were introduced by Otto Struve.

Mr. Marth: I beg pardon. (Laughter). In 1824 Bessel urged upon Fraunhofer that the two half-object-glasses should be made to move on cylindrical surfaces,* but the mechanical difficulties were then considered too great, and Bessel was disappointed that it could not be done.

Lord Lindsay said he would bow to the superior knowledge of Mr Marth on that point.

The President, in returning the thanks of the Society to Lord Lindsay, said it might interest the members to know that it was only by the special protection of the goddess Urania that the instrument was there at all. A serious accident had happened, the whole instrument had come to the ground; if the accident had happened in any other way the probability is that the instrument would have been so far damaged that it would not have been available for Mr. Gill's observations.

Mr. Gill: The instrument has had a very narrow escape.

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* Note by Mr. Marth.—See Bessel's *Astron. Untersuchungen*, I. pp. 49 and 123.

After it had been mounted in this room nearly to the latitude of Ascension, a small final alteration being required, the screw for making the adjustment was turned, when it was found to have run out, and there being nothing to keep down the lower end of the axis, the weight of the heliometer declination axis, &c., raised up to the polar axle box; the polar axis and all it carried were shot out and fell to the floor. The instrument fortunately alighted on the eye-end, which was driven through the floor and snapped off, and by this means the shock of the fall was broken, and no damage which could not be repaired in a few days was done; and now that a new and longer screw has been made we need not fear a repetition of the accident.

Mr. De la Rue wished to say, before the next paper was read, that he was very much struck with the stability of the heliometer. Lord Lindsay had evidently a proper appreciation of rigidity in the mounting of instruments, and had certainly surpassed the ordinary rules of strength. It was worth while calling attention to this, because great stability was not only a comfort to the observer, but also increased the dependence to be put on his observations. Even with the sum voted by the Society towards the expenses of the expedition, except by Lord Lindsay's kindness in lending this instrument, the contemplated observations of Mars would not have been possible. It was a happy concatenation of circumstances that they had been able to find both an observer and an instrument at the opportune moment.

The Astronomer-Royal said, as to the question of the firmness of the instrument which had been alluded to, he should have liked to have seen the declination and polar axes a good deal larger. He pointed these out because they were always the weak parts in the German-mounted instruments. The difficulty had been only partially overcome in the English-mounted instruments. He had seen in Mr. Simm's factory some equatorials which appeared as firm as a rock, but at the same time he should not be satisfied unless he had the axes of the same diameter as for a transit instrument. Instead of having the axes, as here, of 4 inches, he would have had them 8 inches. He had been unable to adjust the eye-piece of Father Secchi's instrument, in Rome, to his eye in consequence of the tremor caused by the smallness of the axes. He looked upon want of steadiness as a great defect of all modern foreign instruments which he had seen.

The Astronomer-Royal subsequently stated that he did not intend in these remarks to impugn the accuracy of the results which would be given by such an instrument, for which purpose he considered it to be perfect. In the heliometer, as in other double-image instruments, a slight unsteadiness produces no bad consequences.

Lord Lindsay said he had seen Padre Secchi's instrument and some others, and none of them had any stability. There were certain good things about them, but he preferred the English mounting. With regard to this instrument he did not think the Astronomer-Royal would find any difficulty with it, if he were to test it practically.

Mr. Gill said: Before reading my short paper I must say a few words in defence of my instrument and of myself. First as to the instrument—there is an old Scotch adage which says "the proof o' the pudding is the prein o' t," and my own experience during three years is that no more stable instrument could be desired. As Lord Lindsay has already said, the heliometer part is the work of Repsolds, of Hamburg, and it would be hard to conceive anything much better than it has proved to be, but I think he did wisely in going to Cooke's for the mounting. Repsold insisted that a strong declination axis should be used, but I do not think he anticipated so strong an axis as we actually employed.

When I saw the mounting which Repsold made for the same instruments in Hamburg, I said "Surely this is not strong enough," they referred me to Otto Struve and to his experience of a similar mounting made for Pulkowa. I found at Pulkowa that they were perfectly satisfied with this mounting, and Lord Lindsay's axis is nearly twice as strong. To double the diameter of this axis seems to me to be overdoing stability in a portable telescope, for it must not be forgotten that double the diameter means eight times the weight, and that, in an instrument already so heavy, is a serious consideration. Indeed, I am sure of this, that if the expedition to Ascension is not successful the fault will not lie with the instrument, and certainly not with its want of stability.

As to the accident, I wish to explain that though I consider myself responsible for it, I am not to blame. Anyone looking at the elevating apparatus would think that there would be four inches of remaining lift, and, the mounting being described as "universal equatorial," no suspicion was entertained that the screw was so short. I am only thankful that the precaution was taken to set the instrument for the exact latitude of the place of observation, as otherwise the accident would undoubtedly have occurred when it could not have been repaired.

Mr. Gill then proceeded to read a paper *On the opposition of Melpomene as a means of determining the solar parallax.* He pointed out that the opposition of the planet occurred under circumstances favourable for observation at Ascension, and did not interfere with the observation of Mars and Ariadne proposed at last meeting. He also drew attention to the opposition of Iris as affording an excellent opportunity for the employment of Dr. Galle's method, viz., measuring the displacement in declination. The declination of Iris would be + 24°, so that the altitude of the planet would be nearly the same for the southern and the European observatories, and that if good observations were made he did not doubt the results would be valuable.

The declination of the planet was too far north for satisfactory observation by the heliometric diurnal method, unless corresponding observations were made at a northern observatory.

Mr. Ranyard read a paper by General Meigs *On a method of making a pendulum swing in an approximately cycloidal arc.* The pendulum rod is supported by two flat springs which, as the pendulum swings, strike against a pin and cause the pendulum to rotate about its axis. The centre of gravity of the pendulum is thus slightly raised at the end of each swing, and by properly arranging the length of the springs and their distance apart the arc can be made approximately cycloidal.

Mr. Ranyard read a paper by Mr. Proctor *On the approaching opposition of Mars.* Want of leisure had prevented Mr. Proctor from preparing, as he had intended, a paper indicating the probable appearance of Mars on different days, but Mr. Marth's Ephemeris, published in the last number of the *Monthly Notices,* in a large degree subserves the purpose of such charts, and it is easy, from the data supplied, to make orthogonal projections for any day and hour. Mr. Proctor expressed his agreement with Dr. Terby, of Louvaine, as to the doubtful features of his map of the planet. Several of the doubts suggested by Dr. Terby are indicated in Mr. Proctor's maps by dotted outlines.

Mr. Glaisher read a note by Mr. Proctor *On the distribution of the fixed stars.* Von Littrow has shown that when only the numerical relations of stars of various orders of brightness are considered, the increase in the number of the fainter stars corresponds with the theory that apparent magnitudes depend chiefly on distance. When, however, the localisation of stars of various orders of brightness is considered, we find evidence of the numerical superabundance of stars really smaller than their neighbours. To take a rough illustration of the effect of this consideration, if in examining any telescopic field taken at random, we find for each increase of illuminating power an increase in the number of stars, the inference is, that we are penetrating further and further into space; but this inference must be rejected if we find that the field of view lies in either of the nubeculæ, and that the increase is not recognised, or only occurs in a markedly less degree, outside the borders of the star cloud; in like manner we find evidence of streams of stars such as are found in various parts of the Milky Way. Numerical increase with increasing powers, if found within such clouds, and in markedly less degree outside their borders, cannot be referred to increase of distance, but, according to the laws of probability, must be explained as due to the recognition of stars, really smaller, lying within the limits of such clouds or streams—for we cannot suppose that they all represent spindle-shaped masses or cylinders, turned in every case exactly towards us.

Captain Noble read a paper communicated by M. de Boë, *On a method of destroying the vibrations in a mercurial reflecting trough.* M. de Boë's plan appeared to be that he immersed a daguerreotype plate in the mercury just below its surface. Captain Noble said the Society would recognise Professor Pritchard's mercurial trough which was described at the December meeting, 1875. In concluding the paper Captain Noble remarked that there was a little knot of astronomers doing good work about Antwerp, Ghent and Malines, in Belgium, among them was the Baron Von Ertbon, who had sent a short paper to him *On observations of Venus.* The Baron had made observations of the dark spots on the planet, and had come to some definite conclusions on the subject of the atmosphere of Venus. He would not read the paper because it had already been read before the Royal Academy in Belgium, but he would lay it on the table so that any one might see it. It might interest Mr. Brett to know that the Baron was firmly convinced that Venus was surrounded by a thick atmosphere which prevented its true surface from ever being seen.

Mr. Lecky mentioned that he had taken an exceedingly good altitude of the sun from Cornhill. By making use of a surface of glycerine, there was absolutely no tremor, the trough required to be blackened so that there should not be a double reflection.

Mr. Dunkin asked whether the observation was made upon the ground or upon the top of the house?

Mr. Lecky: Of course a great deal depends on the asphalte pavement. It was made on the top floor of 80, Cornhill, about 10 a.m.

Mr. Ranyard read a paper by Messrs. Wilson and Seabroke *On the measurements of double stars observed at the Temple observatory in* 1876-7.

Mr. Dunkin said the only fault in the paper was in not giving the right ascension and polar distance. It is a great advantage to have the approximate right ascension and polar distance given in catalogues of double stars, as it is not always convenient to have to refer to a second catalogue.

Mr. Green produced some paper forms which he had specially prepared for the purpose of making drawings of the planets Mars and Jupiter. The lights could be taken out with a knife, and by the aid of a lead pencil and stump very delicate effects could be rapidly produced. He presented a number of forms to the Society for distribution amongst gentlemen who would undertake to make drawings.

Mr. Christie explained a paper by Professor Zenger *On a new solar eye-piece.* He said that he had not seen the instrument, but it appeared to consist of two prisms placed together, so that the light of the sun would be received greatly enfeebled after reflection at the bounding surface between the two prisms, the second of which was of slightly denser glass than the other. The other surfaces of the prisms were curved, so that the combination formed an eyepiece as well as a diagonal reflector. In practice he thought it would be necessary to cement the two glasses together.

Mr. De La Rue: That would materially alter the conditions of the reflected ray.

Mr. Christie said: Yes. He did not see in the paper any provision for cementing the prisms, that was only a suggestion of his own.

Mr. Ranyard said that he believed there was a considerable range of refractive index which could be obtained in Canada balsam, so that a cement might be employed which was intermediate in refractive index between that of the two glasses.

The following papers were also laid on the table and partly read:—

Lunar eclipse observed at Cadiz on 27th February, 1877: Senor A. T. Arcimis.

A table for facilitating the determination of mean equinoctial time to the end of the present century: Admiral Sir C. Shadwell.

Note upon an alleged fall of an aërolite at Supino, Italy: Padre Secchi.

Extracts from letters received relating to the search for the supposed intra-mercurial planet: The Astronomer-Royal.

A determination of the semi-diameter of Venus at the mean distance of the sun from the earth: A. W. Downing.

On a general method of treating the lunar theory: E. Neison.

Elements of the orbit of comet 3 (*Swift's*), 1876: Professor C. Pritchard.

The meeting adjourned at a quarter to ten o'clock, till Friday, the 8th of June.

REVIEWS.

Atlas Celeste. Comprenant toutes les Cartes de l'ancien Atlas de Ch. Dien, Rectifié, augmenté, et enrichi de cartes nouvelles des principaux objets d'études astronomiques: étoiles doubles, multiples, colorées, nebuleuses, et groupes stellaires, mouvements propres des etoiles, etc., par Camille Flammarion, Astronome, ancien membre de l'Observatoire de Paris, etc. Troisième édition. Paris, Gauthier-Villars, 1877.

Charts A and B, both double, give the figures of the constellations in the two hemispheres; these are followed by 23 maps of the constellations on a conical projection, the development of a sphere 25.6 inches in diameter. Chart 24 (double) represents the constellations near the south pole; it contains the 7300 stars of the Cat. of Brisbane. On 25 (double) are laid down the secular proper motions of many stars whose positions are given for 1880. 26 (double) shows the distribution of the multiple stars (exclusively) to the number of 10,487. 27 represents 415 multiple stars, whose relative motions have been ascertained. On 28 we have the orbits of some double stars, and the most curious groups of stars in the heavens. And, finally, 29 has drawings of the finest nebulæ. Such are the contents of this atlas, which comprises 100,000 stars to the 9th magnitude inclusive, and nebulæ. Double and multiple, variable, and periodic stars are denoted by certain marks. It contains nearly all the stars of the catalogues of Lalande, the two Herschels, Piazzi, Harding, Struve, Bessel, Groombridge, and Argelander (not his later catalogues); and for the southern stars, of Lacaille and Brisbane. The epoch is the equinox of 1860, and a small table of precessional corrections for the insertion of new stars and planets is given. This edition has been carefully revised; the number of notable corrections and additions amount to above a thousand, so that the atlas may be said to have been entirely recast. The editor expresses his obligations, amongst others, to MM. Paul and Prosper Henry, of the Paris Observatory, who had noted errors whilst constructing their ecliptic charts. Many omissions and errors, he says, doubtless remain, and he earnestly requests persons who may notice them to communicate with him for the sake of future editions. M. Flammarion hopes shortly to publish as companions to the charts, catalogues, 1, of double stars for instruments of moderate power; 2, Binary stars certainly ascertained to be such; 3, Proper motions; 4, Variable and periodical stars; 5, Red stars. A particular description of each of the charts is given in the Introduction.

The Greek letters and Flamsteed's numerals are affixed to the principal stars, and the boundaries of the constellations marked. The stars are represented by black dots of different sizes, besides distinctive marks for the first four magnitudes. With regard to the scale of the maps, it does not differ much from the large maps of the S. D. U. K., which are the sides of the circumscribing cube to a globe 2 feet in diameter, and form together an area of 24 square feet. We find the area covered by the maps of this atlas to be nearly 21½ square feet, and we have not the distortion that exists in the edges and corners of the former. The Society maps mark stars of the 9th magnitude, but only a fractional part of the immense number; to say nothing of the frequent omission of larger stars. For the sake of comparison we counted (approximately) the objects marked in the constellation of Canis Minor in both works, and found that while 48 are shown on the Society's maps, there are above 240 in the atlas. Again, taking the space from V. h. to VI. h., and +5° to —10° in Orion, we found upwards of 80 in the S. maps, and more than 400 in the atlas, which is thus the richer by 5 to 1. A space of 25 square degrees, void of stars, to the left of Orion's belt in the S. maps (and other similar blank spaces occur) is sprinkled by more than 30 little dots in the atlas.

The planispheres A and B, on which are delineated the figures of the the constellations, are crowded with all the incongruous objects with which various modern astronomers have scribbled over the face of the heavens; reminding one (in the southern heavens) of a curiosity shop, where second-hand optical and mathematical instruments, stuffed birds and fishes, &c., are seen all jumbled together. They are not badly drawn, but the modern cast and ornamentation of the ancient figures contrast with the simplicity of the Ptolemaic designs. We could wish them replaced by the excellent drawings in the Society maps. Those who, like ourselves, have a partiality for cats, may be glad to see one in the southern heavens, just beneath α Hydræ. It appears to have just overturned an air-pump, and to be making off in some alarm, in hopes perhaps of escaping the consequences by getting on board of the ship Argo. On chart 25 arrows indicate the amount and direction of the proper motions in 50,000 years. The stellar perspective due to the displacement of the solar system in space, is represented by isolated arrows. Double stars with common proper motion have a circle round them. In 26 each double or multiple star is represented by one dot, without any distinction of magnitudes. Stars whose orbital motion is certain and rapid are surrounded by a circle. The introduction contains interesting considerations on this map of the distribution of multiple stars. Chart 27, multiple stars with ascertained relative motions, is very elaborate and interesting. On 28 are delineated the orbits of the binary stars, γ Virginis, 70 Ophiuchi, η Coronæ; ξ Ursæ Majoris, and ζ Herculis, which M. Flammarion has determined with great care. There are also small maps giving the detail of the Hyades, Pleiades, Præsepe, 1360 (M 35) in Gemini, &c. 29 is a fine plate representing the most remarkable nebulæ, soft white on a dark blue ground. Occasionally some of the very minute dots are faint or dubious; but this must be difficult to avoid, and probably varies in different impressions. Such might be retouched with ink after examination with a magnifying glass.

We are not aware of the existence of any English atlas that can compete with this one. The charts, engraved on copper, are very clear and distinct. It will be found a most valuable aid to the study of the heavens, and many an amateur might conduce to the improvement of future editions by noting omissions and errors. That there must be many lacunæ there can be no doubt. We are not aware what the present estimate may be, but in 1850 Argelander made the number of stars to the 8th magnitude inclusive amount to 58,000, and he, as well as Struve, reckoned the number from the 1st to 9th magnitude inclusive to be about 200,000, or twice the amount said to be included in this atlas. So immensely does the number increase with each decrease in magnitude. A work of this magnitude can only be perfected by degrees, and those concerned with stellar astronomy will feel greatly indebted to the laborious pains taken by M. Flammarion for the many corrections, improvements, and additions which distinguish this 3rd edition. Nor must we forget what is due to M. Gauthier-Villars for the handsome form in which this valuable folio has been turned out. We trust that both editor and publisher may be rewarded by the appreciation of astronomers, and by a rapid and extensive circulation.

Two Reports on the Royal Observatory, Edinburgh. (A.) Report to the Royal Astronomical Society, London. (B.) Report to the Meteorological Society, London.

From these reports, issued March 20th, 1877, amongst other particulars we learn that the Government grant to the Royal Observatory, Edinburgh, during the past year for all salaries and office expenses was under £1,100. Mr. Alexander Wallace, M.A., and Mr. Thomas Heath, B.A., are praised for their steady and untiring devotion by day and night in the calculations for the Edinburgh Star Catalogue. A large spectroscopic instrument has been constructed in France at the astronomer's private expense. It was long urgently needed, and the necessary funds could not be obtained from any other quarter. Creditable as this is to Prof. Piazzi Smyth, it is to be regretted that more liberality is not shown by Her Majesty's Government to the Edinburgh Observatory.

The portion of the above mentioned sum devoted to meteorology is under £150 *per annum*. The meteorological reductions are on a very large scale. It is mentioned that Mrs. Piazzi Smyth has commenced a little meteorological and spectroscopic journal, arranged chiefly with regard to spectral indications of rain, and it is hoped that after a year's continuance the results may be found interesting. On the photography of *clouds* Prof. Piazzi Smyth says—"One particular series—*before*, *during*, and *after* a solar eclipse—is, perhaps, the most noteworthy; for, although the eclipse was a partial one astronomically, and even that was hid from view at the place by clouds and rain, yet the effect of the cosmical phenomenon, in altering the physical constitution of the earth's clouds for the time being, was abundantly and even grandly manifested on the photographic plate."

Royal Observatory, Edinburgh:
January 25, 1877.

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1. *On the Minute Structure of the Solar Photosphere.* 2. *On the Comparison of Certain Theories of Solar Structure with Observation.* By S. P. Langley, with a Plate. (From the American Journal of Science and Arts. February, 1874, and March, 1875.).

The second of these papers is referred to by Mr. Proctor in his "Sun." The first is also accompanied by a typical drawing of the solar structure, made with the equatorial of the Allegheny Observatory of 13 in. aperture, furnished with suitable micrometers. It is an elaborate paper from an able observer, from whom we may expect further valuable contributions on solar physics. In his recapitulation Mr. Langley says—"The ultimate visible constituents of the solar photosphere being not the rice-grains, but smaller bodies which compose them, and the size of these latter being valuable at not over 0.″3; from a comparison of the total area covered by them with that of the whole sun, we are entitled to say that the greater part of the solar light comes from an area of not over one-fifth of its visible surface, and which may be indefinitely less. *We must, then, greatly increase our received estimates of the intensity of the action to which solar light (and presumptively its heat and actinism) are due, on whatever theory we form them.*" He thinks that the evidence of cyclonic action is more widely marked than that of the eruptive theory, and that we "cannot withhold a more or less complete acceptance of the theory of Faye as one unquestionably resting upon a *vera causa*, and uniting beyond any other a wide range of otherwise isolated truths under a single law."

Results of the Astronomical Observations made at the Royal Observatory, Greenwich, 1874. (Extracted from the Greenwich Observations, 1874.)

This volume of the ever welcome and valuable "Results" contains the following:—1. Catalogue of concluded mean Right Ascensions and North Polar Distances for 1874, Jan. 1, of stars observed in the year 1874, with the annual variations, &c. The number of stars is 1051. 2. Horizontal and vertical diameters and right ascensions and North Polar distances of the sun, moon, and planets, deduced from the observations and compared with the corresponding results of the *Nautical Almanac*, &c. The apparent error of the *Nautical Almanac* in the moon's R. A. seldom amounts to + 1 sec., and exceeds this by a very little, only five times. In N. P. D. the greatest apparent errors are + 7.″56,—7.″71, and generally much less. The apparent errors corresponding to these by the altazimuth are +9″·14 and —4″·19, and the greatest shown by that instrument are +10″·87 and —10″·18. Upon the whole, then, the moon's tabular place is rarely found in error so much as a quarter of a minute of arc, more rarely a little above that, and usually much less. Of the minor planets, 51 were observed either at Greenwich or Paris. We may instance among those whose tabular places are most in error, Cassandra (114), more than 48 sec. R. A., and about 4′ N. P. D.; Lachesis (120) above 20 sec. R. A., and nearly 3′ N. P. D.; Antigone (129) above 40 sec. R. A., and above 1′ N. P. D. The app. error of the *N. A.* in the tabular place of Uranus is between 11 and 12 sec. R. A., and about half a minute of arc N. P. D. 3. Observations of γ Draconis with the Reflex Zenith Tube. The greatest concluded mean zenith distance north is 99.″78 ; the least 94.″52. The mean of which, 97.″15 corresponds closely to most of the other observations. 4. Observations of the solar eclipse of 1874, Oct. 9, with the equations deduced from the differences of Right ascension, and from the difference of North Polar distance of the limbs and cusps. 5. Eclipses, &c., of Jupiter's satellites compared with the *Nautical Almanac*, and occultations of stars by the moon, &c. Of the phenomena of Jupiter's satellites 47 observations were made, and 12 occultations of stars by the moon. Lastly, we have measures of areas of spots and faculæ upon the sun's disc, taken with the photoheliograph. These extend from July 21, 1873, to Nov. 20, 1874. The number of pages in this volume is 102, being 22 more than in 1873.

Mr. Jacob Ennis, A.M., of Philadelphia, author of "The Origin of the Stars," &c., has sent us a *Lecture on the Natural Sciences, and their place in the Public Schools and among the Industrial Arts.* This lecture was delivered in November, 1876, before the Teachers' Institute of Philadelphia. An experience in tuition of thirty years enables Mr. Ennis to treat his theme with effectiveness. His ideas appear to be good and feasible; and though he has especially in view the institutions and circumstances of his own country, much that he says might be useful to masters and teachers here or anywhere. And he is one who sees more in the universe than matter and face. "Mind," he observes, "is the great controlling power of the universe. Mind contrived and constructed all the wonderful objects in our centennial exhibition. The mind of the Omnipresent Deity controls all the physical forces, and these move all creation."

A Government Commission has been deliberating on certain works with a view to increase the effectiveness of the observatory at Arcetri, near Florence. It is also proposed to establish a meteorological society on the summit of Fiesole, nearly 1000 feet above the level of the sea, which it is thought will be of great use. The bishop of Fiesole, Monsignor Luigi Corsani, who has already founded a physico-chemical chair at that place, has offered to contribute to the expense, and the Italian Alpine Club has engaged to do the same.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

THE RING OF LINNÉ.

Sir,—It may not be uninteresting to mention, in corroboration of Mr. Backhouse's letter on p. 125 of the current number of the *Astronomical Register*, that I myself saw Linné distinctly and beautifully as a ring or crater, on the night of April 19. The following is copied verbatim from the note made in my observatory book at the time:

"Thursday, April 19. The Moon 9h. 50m. to 10h. L.M.T. With powers of 154 and 255 I see Linné as a well-defined crater, much in fact as figured by Carpenter in the frontispiece to Vol. VI. of the *Astronomical Register*, save that the shadow of the outside wall is not visible, as there shown, presumably from the greater altitude of the sun. Moon's age, 6·17 days."

It only remains to add that the observation was made with my (Ross) equatorial of 4·2 inches aperture and of 61 inches focal length.

Forest Lodge, Maresfield,
Uckfield: May 11, 1877.

I have the honour to be Sir,
Your most obedient servant,
WILLIAM NOBLE.

STAR IN CANIS MAJOR POSSIBLY VARIABLE.

Sir,—The star Lalande 14088 (Canis Major) is possibly variable. It is marked only 9m. in Harding's charts (1822), but is now about 6½m., and equal to one slightly brighter than Lalande 14105 (closely south of it), which was rated 6m. by both Lalande and Harding.

The position of Lalande 14088 is, for the beginning of 1877, R.A. 7h. 8m. 11s., N.P.D. 112° 27′ 35″.

Umballa, Punjab:
Jan. 22, 1877.

Yours faithfully,
J. E. GORE.

NEW VARIABLE STAR IN LEO MINOR.

Sir,—A little *s. p.* 40 Leonis Minoris Harding marks with a line a 6m. star, which is not in Lalande's catalogue. Failing to see this star with a binocular field glass, which shows very small stars, I examined its place a few nights since with 3-inch refractor, and found a star of about 10m. close to the place given by Harding, about 15′ *s. p*, a 7m. star Lalande 20·612. The star is probably a remarkable variable, and should be watched. The fact of its being marked 6m. by Harding, who, it is well known, compared his charts with the heavens, and particularly its being underlined by him, would seem decisive as to its variability. Is the star in any of the modern catalogues or lists of variables?

Yours faithfully,
J. E. GORE.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN JUNE, 1877.

BY W. R. BIRT, F.R.A.S., F.M.S.

BESSEL. Observations of this crater at *sun-rise* and *sun-set* upon it will be of considerable value at the present time. On the 4th of March, 1797, Schröter observed its shadow at sun-rise to have two projections towards the east, one from the southern part of the wall of Bessel, the other from the northern; these he measured and found the peaks casting them to attain the height of 1017 English feet, being 115 feet higher than the general level of the wall, which he had previously determined to be 902 feet. A drawing of the shadow will be found in Schröter's Fragments T. LXXII., fig. 63. On the 25th of July, 1868, Birt observed the shadow of Bessel at an earlier part of the luni-solar day as it was a little longer than shown by Schröter, the terminator at the time of Birt's observation passed through *Aristoteles* and *Eudoxus*. Mr. Newall, of Gateshead, writes under date of March 8, 1877, to the effeet that "Bessel casts a curious shadow shortly after sun-rise; there are two horns, one on each side of the shadow, and these appear to be cast by *two peaks on the crater ring*, for I saw them at sun-set when Bessel was just on the terminator, and the shadow of the ring was cast the other way." So far as is known at present the observation at sun-set is *unique*, but it is sufficient to settle the question of the *two* peaks existing on the ring given by Schröter and quoted by Lohrmann. Birt's drawing of the shadow of Bessel is given in his Monogram of the *Mare Serenitatis*, p. 3, note in I A ω, the northern peak is registered as I A ω 10, the southern as I A ω 11.

An interesting object in Bessel is the central hill which is but rarely seen. The first notice we have of it is in Stieler's Hand Atlas, date 1860. Bessel is quoted as having a very [small] central height. In 1865, August 30, 9h. 0m. the Rev. J. W. Webb writes: "There is in Bessel a very evident central hill, marked by a similar shadow as described above, it does not look at all like local coloring. The shadow was not very black."

1865, September 7. Mr. Webb observed Bessel with Mr. Bird's silvered glass Newtonian. He saw no trace of a central hill, but some appearance of a low terrace.

1866, January 5, 5h. 20m. Mr. Webb records "a decided central hill marked out by shadow, but the definition is hardly adequate."

1866, April 21, 8h. 30m. to 10h. 40m. Mr. Webb observed a narrow terrace at the inner foot of the east wall.

1867, August 20. Mr. Buckingham and Mr. Jackson-Gwilt saw the central hill in Bessel with the 21-inch refractor.

1870, January 9. Terminator just clear of Aristillus and Stöffler. Mr. Birmingham records "that a central hill appeared in Bessel with sufficient plainness. There was a shadow curving along the base of the wall and dividing it from the illuminated floor which probably sinks down at the edge. From all that I saw I was led to compare the interior of this crater to the appearance of an inverted saucer, with a half globe placed on the flat centre."

The above are the only five instances of a record of a central hill in Bessel. We shall be glad to receive further and fuller records of this interesting crater, with notices of the appearance of the white streak on which it is situated.

We would just mention that, from Mr. Buckingham's observation, the interior of Bessel is somewhat similar to the interior of Plato, being depressed at the foot of the wall.

Hawkenbury, Buckhurst Hill:
May 14, 1877.

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF JUPITER.

Greenwich Noon. 1877.	Angle of position of ♃'s axis.	Longitude of ♃'s central meridian.		Latitude of Earth above ♃'s equator.	Equatorial diameter.	Defect of illumination.
	°	°	diff.	°	″	′
June 1	358·66	87·8	870·7	—2·28	45·52	0.05
2	358·71	238·5	·7		45·58	·05
3	358.77	29·2	870·8	—2·28	45·64	·04
4	358·82	180·0	·7		45·70	·04
5	358·88	330·7	·8		45·75	·03
6	358·93	121·5	·7		45·80	·03
7	358·99	272·2	·8		45·84	·02
8	359·04	63·0	·7		45·88	·02
9	359·10	213·7	·7		45·92	·02
10	359·15	4·4	870·8	—2·28	45·96	·01
11	359·21	155·2	·7		45·99	·01
12	359·27	305·9	·8		46·02	·01
13	359·33	96·7	·7		46·05	·01
14	359·39	247·4	·7		46·07	·01
15	359·44	38·1	·8		46·09	·00
16	359·50	188·9	·7		46·11	
17	359·56	339·6	870·7	—2·28	46·13	
18	359·62	130·3	·8		46·14	
19	359·68	281·1	·7		46·14	
20	359·74	71·8	·7		46·15	
21	359·80	222·5	·7		46·15	
22	359·86	13·2	·8		46·15	on foll. side.
23	359·92	164·0	·7		46·14	
24	359·98	314·7	870·7	—2·28	46·13	·00
25	0·04	105·4	·7		46·12	·01
26	0·10	256·1	·7		46·11	·01
27	0·16	46·8	·8		46·09	·01
28	0·22	197·6	·7		46·07	·01
29	0·28	348·3	·7		46·05	·01
30	0·34	139·0	·7		46·02	·02
July 1	0·40	289·7		—2·27	45·99	0·02

To find the longitude of ♃'s central meridian for

h.	°		h.	°	
7	add 254·0	to the longitude at	12	add 75·4	to the longitude at
8	290·2	noon.	13	111·6	noon.
9	326·5		14	147·9	
10	2·8		15	184·2	
11	39·1		16	220·5	

A. M.

ASTRONOMICAL OCCURRENCES FOR JUNE, 1877.

DATE.			Principal Occurrences.	Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m.
Fri	1		Sidereal Time at Mean Noon, 4h. 40m. 17·63s.			Arcturus 9 28·2
Sat	2	11	Conjunction of Moon and Mars 2° 22′ S. Sun's Meridian Passage 2m. 16·73s. before Mean Noon			9 24·3
Sun	3	17 11 14 12 15 6	☾ Moon's Last Quarter Occultation of 82 Aquarii (6) Reappearance of ditto	**1st** Ec. D.	15 28 26	9 20·4
Mon	4	10	Conjunction of Moon and Saturn, 3° 37′ S.	**1st** Sh. I. **1st** Tr. I. **1st** Sh. E. **1st** Tr. E.	12 36 12 57 14 51 15 13	9 16·4
Tues	5			**3rd** Tr. I. **1st** Ec. D. **3rd** Sh. E. **3rd** Tr. E. **1st** Oc. R.	19 17 9 56 56 10 37 11 59 12 31	9 12·5
Wed	6			**1st** Sh. E. **1st** Tr. E **2nd** Sh. I. **2nd** Tr. I.	9 20 9 39 13 42 14 20	9 8·6
Thur	7					9 4·6
Fri	8			**2nd** Oc. R.	11 16	9 0·7
Sat	9	21	Conjunction of Moon and Mercury 9° 23′ S.			8 56·8
Sun	10	9	Saturn at quadrature with the Sun			8 52·8
Mon	11	2 32 20	● New Moon Conjunction of Moon and Venus 3° 47′ S.	**1st** Sh. I. **1st** Tr. I.	14 29 14 41	8 48·9
Tues	12			**1st** Ec. D. **3rd** Sh. I. **3rd** Tr. I. **1st** Oc. R. **3rd** Sh. E. **3rd** Tr. E.	11 51 11 11 52 12 34 14 15 14 36 15 16	8 44·9
Wed	13			**1st** Sh. I. **1st** Tr. I. **1st** Sh. E. **1st** Tr. E.	8 58 9 7 11 14 11 23	8 41·0
Thur	14		Sidereal Time at Mean Noon 5h. 31m. 32·89s.	**1st** Oc. R.	8 41	8 37·1
Fri	15		Illuminated portion of disc of Venus=0·983 Illuminated portion of disc of Mars=0·869	**2nd** Ec. D. **2nd** Oc. R.	10 33 30 13 30	Jupiter. — 12 19·2

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Jupiter.
Sat	16	11 16	Occultation of 37 Sextantis (6)			12 14·8
Sun	17	18 24	☽ Moon's First Quarter	**2nd** Sh. E. **2nd** Tr. E.	8 24 8 31	12 10·3
Mon	18		Sun's Meridian Passage 0m. 50·87s. after Mean Noon			12 5·8
Tues	19	12	Opposition of Jupiter and Sun Saturn's Ring: Major axis=39″·58 Minor axis=0″·45	**1st** Oc. D.	13 43	12 1·3
Wed	20			**1st** Tr. I. **1st** Sh. I. **1st** Tr. E. **1st** Sh. E.	10 50 10 52 13 6 13 8	11 56·8
Thur	21			**1st** Ec. R.	10 25 51	11 52·3
Fri	22	12 46	Occultation of 4 Scorpii (6)	**2nd** Oc. R.	12 57	11 47·9
Sat	23	9 9 9 32	Occultation of B.A.C. 5603 (6½) Reappearance of ditto	**3rd** Ec. R.	8 37 53	11 43·4
Sun	24	17	Conjunction of Moon and Jupiter 4° 53′ N.	**2nd** Tr. E. **2nd** Sh. E.	10 46 11 1	11 38·9
Mon	25	4 52	○ Full Moon			11 34·4
Tues	26					11 29·9
Wed	27	12 54 13 55	Occultation of B.A.C. 7049 Reappearance of ditto	**1st** Tr. I. **1st** Sh. I. **1st** Tr. E. **1st** Sh. E.	12 34 12 46 14 50 15 2	11 25·4
Thur	28	14 4	Near approach of 30 Capricorni (6)	**1st** Oc. D. **1st** Ec. R.	9 53 12 20 21	11 21·0
Fri	29			**1st** Tr. E. **1st** Sh. E. **2nd** Oc. D.	9 16 9 31 15 11	11 16·5
Sat	30			**2nd** Oc. D. **3rd** Ec. R.	8 52 12 37 27	11 12·0
JULY *Sun*	1	0 18	Conjunction of Moon and Mars 5° 33′ S. Conjunction of Moon and Jupiter 4° 4′ S.	**2nd** Tr. I. **2nd** Sh. I. **2nd** Tr. E. **2nd** Sh. E.	10 14 10 49 13 1 13 37	11 7·6

THE PLANETS FOR JUNE.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	4 3 35	N.17 14½	11″·7	23 19·5
	9th	4 0 30	N.16 21½	10″·3	22 45·0
	17th	4 15 40	N.17 30½	8″·6	22 28·6
	25th	4 49 12	N.19 57	7″·1	22 30·6
Venus ...	1st	5 7 13	N.23 12	9″·8	0 26·8
	9th	5 50 1	N.24 2½	9″·8	0 38·1
	17th	6 33 6	N.24 7½	9″·9	0 49·6
	25th	7 15 56	N.23 37	10″·0	1 0·9
Mars ...	1st	22 2 13	S. 14 60	14″·2	17 19·1
	9th	22 18 58	S. 13 50½	15″·4	17 4·3
	17th	22 34 32	S. 12 45	16″·4	16 48·4
	25th	22 48 44	S. 11 46½	17″·8	16 31·1
Jupiter ...	1st	18 4 7	S. 23 7½	42″·9	13 21·6
	9th	18 0 1	S 23 8½	43″·3	12 46·1
	17th	17 55 38	S. 23 9	43″·6	12 10·3
	25th	17 51 13	S. 23 9½	43″·6	11 34·4
Saturn ...	1st	23 25 1	S. 5 51½	15″·4	18 41·7
	9th	23 26 16	S. 5 45½	15″·6	18 11·4
	17th	23 27 9	S. 5 42½	15″·8	17 40·9
	25th	23 27 39	S. 5 41½	16″·2	17 9·9

Mercury rises nearly at the same time as the sun on the first day, and gradually earlier each day, the interval increasing to an hour and a quarter before the sun on the 30th.

Venus is too close to the sun to be well observed, setting on the 1st about forty minutes after the sun, the interval increasing to one hour on the last day.

Mars may be observed half-an-hour after midnight at the beginning of the month. He rises about forty-five minutes before midnight on the last day.

Jupiter rises about an hour and a quarter after sunset at the beginning of the month, the interval decreasing. Towards the end of the month he rises before sunset. He is at opposition to the sun at midnight on the 19th.

Saturn rises rather more than an hour after midnight on the 1st, the interval decreasing. He rises three-quarters of an hour before midnight on the last day, and is at quadrature with the sun on the 10th.

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF MARS.

Greenwich Noon.	Areographical longitude of the centre of ♂'s disc.	∥	Areographical latitude of the centre of ♂'s disc.	Angle of position of southern pole.	Diameter of disc.	Amount and position of greatest defect of illumination.	
1877.	°	°	°	°	″	″	°
June 9	269·8	350·3	—24·19	168·55	12·76	1·72	249·9
10	260·1	·4	24·23	168·20	12·88	1·73	
11	250·5	·3	24·27	167·85	13·00	1·74	249·8

12	240·8	·4	24·30	167·51	13·12	1·75	
13	231·3	·3	24·33	167·17	13·24	1·75	249·7
14	221·5	·4	24·36	166·83	13·36	1·76	
15	211·9	·4	24·38	166·50	13·49	1·77	249·6
16	202·3	·4	24·40	166·17	13·61	1·78	
—							
17	192·7	350·3	−24·41	165·85	13·74	1·78	249·6
18	183·0	·4	24·43	165·53	13·87	1·79	
19	173·4	·4	24·44	165·22	14·00	1·79	249·5
20	163·8	·4	24·45	164·91	14·13	1·80	
21	154·2	·4	24·45	164·60	14·27	1·80	249·5
22	144·6	·5	24·45	164·30	14·40	1·81	
23	135·1	·4	24·45	164·00	14·54	1·81	249·5
—							
24	125·5	350·4	—24·45	163·71	14·68	1·81	
25	115·9	·4	24·44	163·42	14·82	1·81	249·5
26	106·3	·5	24·43	163·14	14·96	1·82	
27	96·8	·4	24·42	162 87	15·11	1·82	249·5
28	87·2	·5	24·41	162·60	15·25	1.82	
29	77·7	·4	24·39	162·33	15·40	1·82	249·6
30	68·1	·5	24·37	162·07	15·55	1·82	
—							
July 1	58·6		—24·35	161·81	15·70	1·81	249·6

To find the longitude of central meridian

	h.		°	
for	12	add	175·2	to the longitude at noon.
	13		189·6	
	14		204·4	
	15		219·0	

The 24 sketches of Mars, made in 1862 and 1864 by Mr. Banks and Mr. Green, and published in Vol. III. of the *Astronomical Register*, p. 71; the 4 sketches by Mr. Dawes, re-published in the same volume, p. 221, and the 16 sketches by Mr. Webb, published in Vol. IV. of the *Intellectual Observer*, p. 182, are arranged in the following list according to the longitudes of their central meridians :

Areographical longitude and latitude of centre of disc.		Diameter.			Times assigned to the sketches.					
°	°	″						h.	m.	
3·0	—21·5	22·9	Green	1	1862	Spt.	25	12	30	Gr.
9·4	— 3·0	17·2	,,	1*	64	Nov.	14	11		
20·5	—26·3	12·5	Webb	8	62	Dec.	8	10	30	
24·0	— 3·0	17·2	Dawes	10	64	Nov.	14	12	0	
30·5	—20·9	22·6	Webb	9	62	Spt.	19	10	45	
44·0	— 8·8	16·4	Banks	1*	64	Dec.	12	5	40	
49·1	— 2·6	17·1	Dawes	12	64	Nov.	12	12	30	
67·9	— 9·5	15·8	Banks	6*	64	Dec.	17	10	20	
69·2	—26·5	13·8	Green	2	62	Nov.	29	8		
71·2	— 1·8	16·7	,,	2*	64	Nov.	7	11		
75·6	—26·5	14·3	Webb	10	62	Nov.	26	6	30	
77·7	— 9·5	15·8	Banks	2*	64	Dec.	17	11		
86·4	—24·8	20·5	,,	2	62	Oct.	23	10	30	
130·9	—26·5	14·6	Webb	11	62	Nov.	24	9		
132·7	— 7·5	17·0	Banks	3*	64	Dec.	5	7	30	

147·9	—26·5	14·6	Webb	12	62	Nov.	24	10	10
154·5	—26·5	15·0	„	13	62	Nov.	22	9	20
157·5	—23·9	21·8	Banks	3	62	Oct.	15	10	30
173·6	—26·5	15·1	Green	3	62	Nov.	21	10	
175·1	—23·1	22·5	Webb	15	02	Oct.	9	8	5
188·1	—26·3	16·3	„	14	62	Nov.	15	7	10
215·7	— 0·3	15·1	Green	3*	64	Oct.	22	11	

(For correction of the error of the month, vide *Astronomical Register*, Vol. XI. p. 181.)

216·9	—23·0	22·6	Webb	16	62	Oct.	8	10	20
244·0	— 6·2	17·3	Green	4*	64	Nov.	29	11	30
244·8	— 5·6	17·4	Banks	4*	64	Nov.	26	9	45
248·7	—22·4	22·9	Green	4	62	Oct.	3	9	30
267·0	—22·4	22·9	Banks	4	62	Oct.	3	10	45
273·3	— 5·1	17·5	„	5*	64	Nov.	24	10	30
274·3	— 5·6	17·4	Dawes	6	64	Nov.	26	11	46
275·7	—26·2	17·2	Webb	1	62	Nov.	10	10	
284·9	—26·1	17·4	Banks	5	62	Nov.	9	10	
285·4	—26·2	17·2	Webb	2	62	Nov.	10	10	40
288·0	— 5·1	17·5	Green	5*	64	Nov.	24	11	30
308·3	—21·9	23·0	Webb	3	62	Spt.	29	11	10†
319·1	—21·5	22·9	Green	5	62	Spt.	25	9	30
324·6	— 4·2	17·4	Dawes	8	64	Nov.	20	11	36
325·2	—25·9	18·1	Webb	4	62	Nov.	5	10	15
332·2	—21·6	23·0	„	5	62	Spt.	26	11	†
332·8	— 3·0	17·2	Green	6*	64	Nov.	14	8	30
341·0	—21·5	22·9	„	6	62	Spt.	25	11	
346·5	—25·8	18·3	Webb	6	62	Nov.	4	11	5
346·8	—26·2	12·3	„	7	62	Dec.	10	9	30
552·2	—25·3	19·5	Banks	6	62	Oct.	29	7	45
354·4	—25·7	18·5	„	1	1862	Nov.	3	11	

* The numbers of Mr. Banks' and Mr. Green's sketches made in 1864 are distinguished by an asterisk from those made in 1862.

† The times of Mr. Webb's sketches, No. 3 and 5, which are not given in his paper in the *Intellectual Observer*, have been kindly supplied by him, as their uncertainty is really not greater than that of the others.

Another list of sketches made in 1862 and 64, of which the present one may be considered a supplement, will be found in the *Monthly Notices* for April, page 305—307.

A. M.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Dec., 1876.
Browning, J.

To Jan., 1877.
Shaw, Rev. J.

To June, 1877.
Elliott, R.
Lewis, H. K.
Linwood, Rev. W.

To Dec. 1877.
Bazley, T. S.
Dobie, Dr. W. M.
Shawcross, W.
Warner, T.

To March, 1878.
Jackson, Rev. W.

To June, 1878.
Sargent, Rev. J. P.

The Astronomical Register is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings** per Quarter, ***payable in advance***, by postage stamps or otherwise.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. JACKSON, *Clarence Road, Clapton, E.*, **not later than the 15th of the Month.**

The Astronomical Register.

No. 175. JULY. 1877.

ROYAL ASTRONOMICAL SOCIETY.

Session 1877—78.
Last Meeting before the long vacation, June 8th, 1877.
William Huggins, Esq., D.C.L., LL.D., &c., *President*,
in the Chair.
Secretaries—Mr. Ranyard and Lord Lindsay occupying
Mr. Glaisher's place.

The minutes of the last meeting were read and confirmed.

Lord Lindsay reported that 54 presents had been received since the last meeting of the Society, amongst them was the second volume of the "Encyclopædia Metropolitana," which contains Sir John Herschel's essay on light.

Major-General John Baillie, 25, Hamilton Terrace, St. John's Wood.
Thomas Brewin, Esq., 14, St. Nicholas Street, Leicester.
G. Calver, Esq., Hill House, Widford, Chelmsford.
James Campbell, Esq., F.R.G.S., Arkley House, Barnet.
W. F. Denning, Esq., Tyndale House, Ashley Down, Bristol,

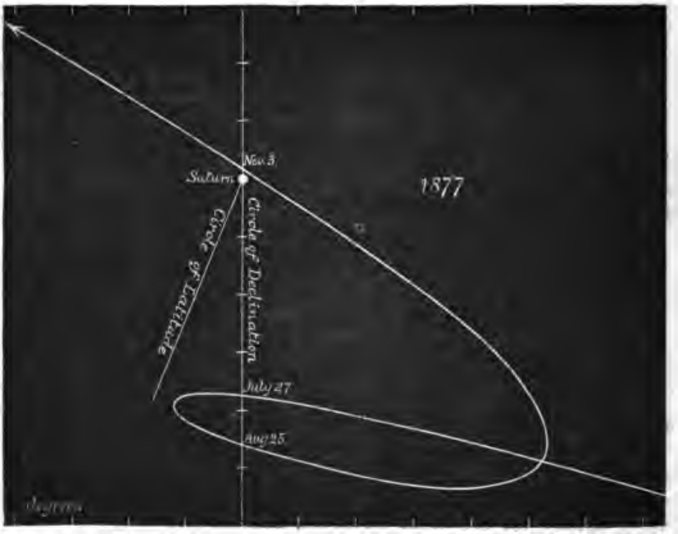
were balloted for and duly elected Fellows of the Society.

The President: One of the names for suspension this evening is that of Mr. Lambert, the gentleman from whom the Society has received £500, part of a sum left by his father to be distributed chiefly for scientific purposes, and I am sure you will be happy to record your votes in his favour, as an expression of your acknowledgment of his selection of this Society as one of the recipients of his father's gift.

The President: I regret that it is my duty to state that the two Fellows whose names have been suspended in the meeting-room, in accordance with the Bye-laws, are to be expelled from the Society; their names are: Mr. J. Garbut and the Rev. T. Tordiffe. They are each in arrear with their subscriptions eight guineas. I therefore pronounce them to be expelled from the Society for non-payment of their subscriptions.

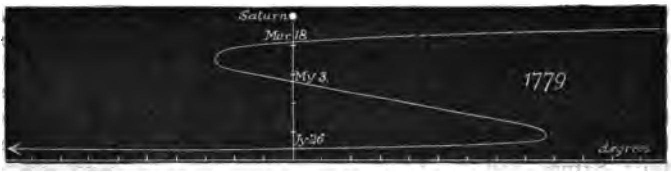
The President: Before proceeding to the papers to be read, I will mention that Mr. Stone, Her Majesty's astronomer at the Cape of Good Hope, has presented to the Society a volume of tables for facilitating the computation of star constants, which he has just published. In a private letter which I have received from him, he requests me to state that he will be happy to receive applications for copies of these tables from any Fellow of the Society to whom they would be of real use. As we shall have to proceed at an early hour to the special meeting we have only a short time now for the ordinary business of the Society. There are three gentlemen who have *viva voce* communications to make to us. I will call on Mr. Marth first.

Mr. Marth: I will occupy only a few minutes of your time in order to explain some diagrams connected with the approaching conjunction of Mars and Saturn. Both planets come into

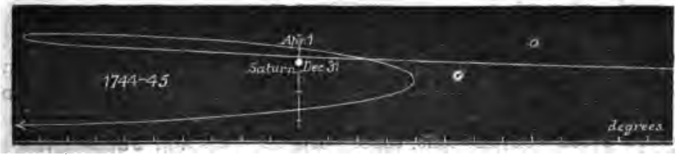


Path of Mars relative to Saturn in 1877.

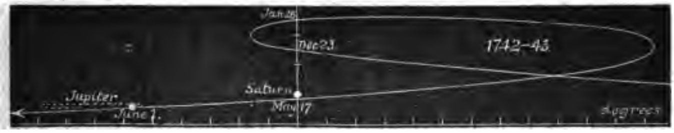
opposition to the sun early in September, and if you look into the *Nautical Almanac* you will find three conjunctions between them given amongst the phenomena. In this diagram there is represented the apparent path of Mars in reference to Saturn considered as a fixed point. On July 27, Mars passes Saturn in right-ascension, reaches on August 10 the most easterly point of the loop, repasses Saturn again in its retrograde course on August 25, and describes then the curve laid down in the figure, and passes on November 3 a third time, and this time close to Saturn. This is the only triple conjunction of the two planets in the nineteenth century. Yet it may be considered doubtful whether strictly it ought to be called such; for that depends on what is to be understood by the term "conjunction." In our modern ephemerides planetary conjunctions are reckoned in reference to right-ascension, and so it is in the present case. But, if they were reckoned, as conjunctions of the moon and planets with the sun are still reckoned, and as formerly conjunctions generally were reckoned, in reference to "longitude," there will be no triple conjunction of Mars and Saturn this year, since the loop line,



Path of Mars relative to Saturn in 1779.



Path of Mars relative to Saturn in 1744-45.



Path of Mars and of Jupiter relative to Saturn in 1742-43. The dotted line represents the path of Jupiter.

which Mars describes, does not reach the circle of latitude passing through Saturn, but turns back while still about 21′ short of it, as the next diagram shows.* Accordingly, in longitude there will be only one conjunction, on November 3, and no triple conjunction in the present century. For the sake of comparison I have prepared other diagrams, showing the paths of Mars relative to Saturn on the three last occasions of triple conjunctions, viz., in 1779, when Mars described a zigzag curve, 1744—45, when it described a long flat loop, and 1742—43, when, while Mars described the large loop, Jupiter moved along the small flat curve in the figure, and the long-continued proximity of the three planets furnished a very interesting popular sight. At the third conjunction, on May 17, Mars passed at about 10′ distance from Saturn, and on June 1 it was in very close conjunction with Jupiter. The planets were observed by Cassini II. and Maraldi II., but the nearest approach was not observable in Europe, as it occurred some hours after setting. An observer in America might have seen the nearest limbs only about 40″ asunder.

Similarly, as in the case of Mars and Saturn, I have represented in another diagram the path of Mars relative to Jupiter on the three last occasions of their being three times in conjunction, namely in 1836-37, 1789-90, and 1646-47. The next occasion preceding this last was in 1503-4, and it was indeed a remarkable one, since during the same apparition Mars was also three times in conjunction with Saturn, so that altogether there were seven conjunctions, three between Mars and Jupiter, three between Mars and Saturn, and one between Jupiter and Saturn. The only observatory then existing, and deserving to be so called, was that of Walter, in Nuremberg, so that there seemed to be not much chance of finding any actual observations or notices of these conjunctions recorded except in Walter's observations. When I saw in the last printed list of presents received by the Society the title of Snell's edition of the Observations of the Landgrave of Hesse, one of the books formerly belonging to Mr. Sheepshanks, I remembered that Walter's observations are to be found in the appendix, and I have, therefore, looked into the book and have not been disappointed in finding some of these conjunctions recorded amongst Walter's last observations.†

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* We have not thought it necessary to reproduce this diagram, as the circle of latitude passing through Saturn on August 10th is given in our woodcut of the first diagram.—Ed. *Astronomical Register.*

† [Note by Mr. Marth.] The title of the book is: Coeli et siderum in eo errantium Observationes Hassiacae . . . . ; quibus accesserunt,

So far about triple conjunctions which are past. Now a word about those to come. I find that the next triple conjunction between Mars and Saturn will take place in the year 1946, so that I cannot well wish the gentlemen present, except the youngest, that they may live long enough to see it. The next triple conjunction between Mars and Jupiter will not occur till 1980. But before these two events, a far rarer event will take place, namely, in 1940, a triple conjunction between Jupiter and Saturn, such as has only once been observed, in 1682-83 by Flamsteed* and Hevelius, which before that has not happened since 1425, and of which only one previous instance has been computed, namely, that occurring in the year 7 B.C., computed by Ideler† and Encke,‡ and also, about twenty years ago, by Professor Pritchard, in a paper in the memoirs of this Society,§

Joannis Regiomontani & Bernardi Walteri Observationes Noribergicæ. Lugduni Batav. 1618. It may, perhaps, not be out of place, if I mention a circumstance in connection with Walter's Observatory, which was recalled to my mind by a late political incident. The erection of this, the first modern observatory which deserves the name, was chiefly due to the beneficent influence of the learning of Regiomontanus, who, in 1471, had settled in the free town. The then King of Hungary, Matthias Corvinus, who appreciated learning, was bringing together a considerable library, and had, in 1469, called Regiomontanus to Buda, in order to preside over it; but the wars, in which the king got involved, frustrated his good intentions, and Regiomontanus had to leave, and was then received with open arms by the free citizens of Nuremberg, especially by Walter. The king's library was afterwards captured and carried off to Constantinople by the Turks, and it is this library (or rather the poor remnants of it still existing) which the Turks have now had the good sense to restore to Hungary. The ephemerides, extending from 1475 to 1506, which Regiomontanus published in 1474, and which were afterwards of such good service to Columbus, Vasco de Gama, and the other great navigators of the time, were dedicated by him to King Matthias.

* [Note by Mr. Marth.] "Whilst the Common People have admired to see the two Superior Planets Saturn and Jupiter continue to near each other this whole year, and our Astrologers have affrighted them with fearful Predictions of direful events to succeed this appearance, the more Judicious are desirous to know how often and at what time their Conjunctions happen. . . . "—Flamsteed in Phil. Tr. vol. xiii. p. 244. Hevelius' account is to be found in the same volume, p. 325.

† [Note by Mr. Marth.] Ideler, Handbuch der mathem. u. technischen Chronologie. 1826. Vol. II. p. 406.

‡ [Note by Mr. Marth.] Ideler, Lehrbuch der . . Chronologie. 1831. P. 429, footnote.

§ [Note by Mr. Marth.] Mem. vol. xxv., p. 119.—In the "Lehrbuch," Ideler had already corrected himself by suppressing the amusingly strained passage about the possibility of the two planets appearing to weak eyes as one star, and by communicating the result of Encke's computations. These results for the conjunctions of the planets are :

the contents of which, however, it is only fair to say, had been anticipated by Ideler himself twenty-five years before.

However, it is, perhaps, of more immediate interest to know what conjunctions will happen in the course of the next few years, and I have, therefore, represented here the close conjunction of Mars and Saturn on June 30, 1879, when they will pass each other at a distance of about 80″, though not while visible to us in Europe. Since telescopes have been in use, there is only one instance known when the occultation or the covering of one planet by another has been observed, and that was the covering of Mercury by Venus, observed in May, 1737, by Bevis, at Greenwich.

The next conjunctions of interest will be those of Mars with Saturn and Jupiter, in July, 1881. In the preceding April Jupiter comes into conjunction with Saturn, as usually in every twentieth year, but very near to the sun and invisibly. But in July Mars will overtake both and the three superior planets, so near each other, will be for several weeks an interesting sight in the mornings. I have represented their mutual positions on five of these mornings in the next diagram. The nearest approach of Mars to Jupiter in the afternoon of July 22, will not be visible in Europe. And now to go back to a very remarkable former appearance of the three planets, I have laid down their positions for some evenings in the autumn of 1604. On September 26, Kepler and some of his disciples had observed the conjunction of Mars and Saturn, Jupiter being about 7° distant. On October 9 Mars was observed in conjunction with Jupiter, as represented in the diagram. On the following evening, October 10, one of the disciples, while looking for them, saw not three but four bright stars. Cloudy evenings intervened. On October 16 Kepler saw the four stars. On October 17 the constellation appeared as laid down in the figure. What was the fourth star? Much brighter than Jupiter and nearly approaching to the brightness of Venus when at her greatest brilliancy? It was the splendid and beautiful New Star of 1604, which though only seen low near the horizon,

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[Note § Continued.]

	Berlin M.T.	Geoc. long.	Lat. of ♃.	Lat. of ♄.
B.C. 7	h. m. s.	° ′ ″	° ′ ″	° ′ ″
May 29	3 45 49	350 58 16	—1 20 52	—2 19 51
Sept. 30	14 1 51	347 27 30	—1 46 4	—2 44 3
Dec. 5	11 35 46	345 35 44	—1 28 3	—2 30 50

and for their oppositions to the sun

	Berlin M.T.	Geoc. long.	Lat. of ♃.	Lat. of ♄.
♄ ☍ ☉ Sept. 13	22 34 29	348 45 2		– 2 44 26
♃ ☍ ☉ „ 14	17 27 8	349 31 52	—1 47 11	

had fired Kepler's imagination, as those acquainted with his works well know.*

I have prepared another diagram referring to an entirely different subject, but will not occupy your time any longer.

Dr. Royston-Pigott was called upon to read a paper *On collimating reversible instruments such as the altazimuth, equatorial, and transit instruments by the destruction of parallax.*

Dr. Royston-Pigott was understood to say, in explanation of the object of the method proposed, that when a very distant object was not easily attainable as a meridian mark, the employment of scales of inches, placed at convenient distances, would enable the observer to correct the collimation or test the squareness of the axis of the telescope to that of the declination circle, in the case of reversible instruments, with great precision. This is accomplished by ascertaining trigonometrically the absolute "throw" of the instrument when alternately reversed, *i.e.*, turned over westwards and eastwards, and directed towards the same object. A spider line vertically placed in the centre of the field of view would thus be seen to cut the distant scale if the collimation were perfect exactly at points, whose distance was the absolute "*throw*" of the equatorial, &c.

And if two scales were employed, both visible in the same field of view, respectively placed (say) 66 and 156 yards distant from the object-glass, the spider line readings on the two scales would be absolutely the same only when the collimation is perfect. Dr. Royston-Pigott also explained his method of bringing into focus the near and distant scale in the same field of view, and at the same instant, by cutting off the aperture of the object-glass, so as to leave a vertical aperture only and about half-an-inch broad.†

The least error in the collimation developes a considerable parallax on the most distant scale on reversing the instruments.

Mr. Dunkin: Dr. Pigott says that by his contrivance he can determine the collimation of his instrument within two seconds of arc, which certainly is not a large quantity, but I should feel myself very much put out if the collimation determined on the transit-circle at Greenwich were not within a quarter of a second of arc.

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* "Quisquis es, qui portentum stupendum oculis tuis non adspexisti, facem imaginare tibi flagrantissimam luminis purissimi, ventis validissimis agitatam et quassatam: talis erat vibratio luminis, talis flammarum ebullitio, talis scintillatio ignivoma rapidissima."—Kepleri Opera. Ed. Frisch. Vol. iv. p. 282.

† [Note by Dr. Royston-Pigott.] A second of arc is represented by an inch disc 206265 iuches,, *i.e.*, $5729\frac{1}{3}$ yards distant, or by a line $\frac{1}{100}$th of an inch broad, placed 57 yards 10 inches distant.

Dr. Pigott: Though I put the error at two seconds, I might have said it is much less than a second. You will find a very fine line at 500 yards distance is a very good criterion. I wanted to know whether I could collimate an instrument without these mercurial reflections and Bohnenberger eye-pieces.

Lord Lindsay: The value of two seconds of arc at 500 yards will be very small indeed. At a mile it would be not quite equal to the diameter of a fourpenny-piece.

The President called upon Mr. Gill to give an account of an instrument which was shown upon the table.

Mr. Gill: I wish to record some of the troubles of a man who desires to determine the solar parallax without systematic errors. I have received several suggestions as to systematic errors that may arise at Ascension. One of these is one which I have endeavoured to combat by two methods. Supposing the image of the star of comparison is placed by the heliometer in the centre of the planet, the planet's light being reduced by the interposition of the wire screen over the segment of the object-glass through which its light passes. If the observer has any personality due to want of perfect spherical form in his eye he will have a tendency to place the image or star too high or lower than the centre of the planet. Most people if they draw a line round a halfpenny and as carefully as they can place a dot at the point which they estimate to be the centre, will find that if they reverse the drawing what was previously estimated as the centre now no longer appears to be the centre. I must endeavour to discover whether any such tendency exists in my own eye, and for this purpose I have had prepared a model representing the planet and a star, which can be reversed, the larger hole can be illuminated so as to represent Mars when seen from a distance, with the instrument kindly placed at my disposal by Lord Lindsay; another hole at the other end represents a small star. The images of the star and planet can be brought together, and I can measure the angular distance from the centre of Mars to the star with the heliometer. Then I can reverse the apparatus, and if the distance and centres remain the same as before, my eye will be proved to have no error arising from want of sphericity, and if there is any difference it will amount to twice the error of a single measure. But one of the most distinguished authorities, though he finds no real objection to the use of this method, thinks that no method will be satisfactory which only imitates celestial observations by a mechanical contrivance. I have therefore adopted a plan suggested by Professor Keyser. It is well known thas the late Mr.

Baily, when he was making experiments on the standard measures of length, never observed the position of the line exactly as anybody else did, he used, therefore, to select as many persons as he could, and get them all to make readings, but Keyser suggested a better idea; he placed in front of the eye-piece a prism of total reflection, so that the rays of light were reflected at right angles, and by turning the prism through 180° the image could be turned upside down, and thus what in the one case would be a plus error would be converted into a minus error, and by taking the mean of the two the whole error will be eliminated. The heliometer will have an eye-piece of this kind, to be used with Mars, so that I can make two measures with my personality in one direction and two in the other, and whether I have a personality or not the measures will be entirely free from all contingency of the kind.

The President: We are obliged to Mr. Gill, and I will only ask if he has any other information to give us before he leaves.

Mr. Gill: I would simply return my thanks to a few individuals who have contributed very cordially to assist me. In the first place, I must express my deep sense of obligation to Lord Lindsay, for he has not only lent an instrument of great value, but he has expended a large sum of money in remunerating me for investigating its constants. He has packed the instrument and sent it here, and he has done everything he could to advance the objects of the expedition, including the loan of a number of chronometers. I must also express my obligations to the Astronomer-Royal for the trouble he has taken in obtaining the assistance of the Admiralty, and to many of the Fellows of this Society who have given me much assistance and advice. I think the thanks of the Society are also due to Mr. Donald Currie, the owner of the splendid line of steamers which conveys me to Ascension, not only for giving me every possible assistance, but for diminishing, very greatly indeed, the cost of transmitting the instrument.

The President: I am sure I need not ask you, gentlemen, to join me in wishing Mr. Gill every possible success (Hear, hear), and at some early meeting next session we shall hope to welcome him back.

Dr. Pigott: I believe I possess a very fine nautical chronometer, and if Mr. Gill would be willing to accept the trust of it, I shall be happy to place it at his disposal.

Mr. Gill: I am much obliged to Dr. Pigott; I will talk with him after the meeting on the subject.

The President then called on the Secretary to read the notice convening the special meeting.

Lord Lindsay having read the notice.

Mr. Ranyard said, that there was a misprint in the notice. The words "Bye-laws 3—14," at the top of the first page, should be Bye-laws 3—12.

The President: I would call the attention of the Fellows to the fact that the only business we can consider this evening is that for which we are specially called together, viz., to consider the special alterations of the Bye-laws, of which notice is given in the circular. The first set of alterations refers to the balloting for officers of the Society, and the two subsequent ones have reference to the duties of the auditors; and I think it would facilitate business if we consider, in the first instance, the first set. It will now be for anyone who wishes to propose that these alterations be made to propose them formally, and if the motion is seconded the matter will be open for discussion.

Lord Lindsay: I have just had in my official capacity, as one of the Secretaries, to read this notice, but acting in my private capacity I shall now proceed to move "That this Special General Meeting be adjourned until the Ordinary Meeting in November." I do not, I need hardly remark, wish to oppose the alteration of the rules in any factious manner, or to stop the proceedings of the Society, but I take this course because the Bye-laws of our Society lay it down that no business shall be entered into at a special meeting except that of which due notice of one month has been given. It will be seen that this circular was sent out on the 26th of May, and this is only the 8th of June. It has, therefore, been quite impossible for any member of the Society to place any amendment upon the paper and be in order to move it. And consequently if we proceed to vote this evening we shall have no opportunity of entering into the expediency of alterations in these suggested alterations. Some of them are very desirable, but others I feel are quite the contrary, and I should feel disposed either to modify them or move their total rejection. But if we go on now nothing can be legally done except either to accept or reject the alterations before us, and consequently, I think, it would be decidedly for the benefit of the Society if this meeting were adjourned until at least the November meeting. It would be, perhaps, still better if the discussion on this question could be deferred till the annual general meeting, because then there will be no necessity for giving notice beforehand for any amendment.

The Rev. J. C. Jackson: I shall be glad to second Lord Lindsay's motion.

Mr. Snell: The mere adjournment of the meeting will leave you where you are; you must either dissolve this meeting altogether, or else discuss the matter which is before you.

Mr. Ranyard: A fresh notice may be given in the meantime. There is no doubt that during the time between this and November any Fellow of the Society can give any notice he pleases, and the Secretaries cannot refuse to receive such notice.

Mr. Dunkin: I beg leave to move as an amendment that we proceed immediately with the business on hand.

Sir. E. Beckett: That is not an amendment.

Mr. Dunkin: I am objecting to the motion.

The President: You can speak on this motion, but your amendment is a direct negative to the motion.

Mr. Dunkin: I will speak then on the motion for adjournment. I cannot for the life of me see any reason why this postponement should be made. These resolutions have been before the Council, and have been before the Society now for the last four months. (Cries of "No, no.") These resolutions have been considered most carefully by a Sub-committee, appointed by the Council, and have been supported by the Council itself, at which our Secretaries were present, and no opposition was made to them. I say it is really begging the question altogether to stamp it out at the very commencement in this manner, and I call upon you, gentlemen, not to allow such a thing. I am an old member of this Society, I have been a member for nearly forty years, and as an old officer of your Society, who has sat at this table year after year, I am ashamed of such conduct. (Cries of Oh!)

The President: We shall be glad to hear any reasons bearing upon the motion.

Mr. Dunkin: I cannot help speaking strongly.

The President: I am sure that keeping strictly to the reasons will tend greatly to accomplish what we all have at heart—

Mr. Dunkin: Then I beg to speak against the adjournment, and I beg of you most earnestly to go on with the business, when I will explain to you, if you will allow me, the reasons why you are called together; you can then say whether you will have the amendments or not. I am not at all wedded to them, and if you do not like them reject them, but it is a great pity to attempt to stamp the question out in this way without consideration. Only think what you are doing. If you adjourn the meeting till November, what will you do then? Why, you will put the alterations aside for twelve months. If you have an adjourned meeting in November you cannot confirm the result till December, and then the time for choosing the new Council will have gone by. I call upon you, therefore, gentlemen, not to vote for the adjournment.

Mr. De la Rue: I did not intend to speak on this occasion, simply because I desired that all things should go on peaceably in the Society, but I am very sorry to see a difference of opinion; I must say that I myself think it would be a very inconvenient proceeding to adjourn a meeting that has been called to take into consideration a set of Bye-laws, which, after due consideration on the part of a committee appointed specially for that purpose, have been been drawn up in the most liberal possible spirit. There was a doubt on my mind after the last annual meeting whether certain proceedings fell strictly within the Bye-laws of the Society, and a difference of opinion has been expressed by eminent lawyers called upon to give opinions. In order to obviate any possible misconception in the future, the committee have in express words proposed to render legal that about which there was doubt. The whole of these Bye-laws are of a most democratic tendency, in so far as they are calculated to infuse continually, as far as possible, new blood into the Council, so that we may have as much as possible new ideas to govern the proceedings of the legislative body of this old and respected Society. I think, therefore, that it would be far better if you would not vote for the adjournment, but either accept or negative the propositions before you. If you do not like the Bye-laws as they are laid before you, simply vote against them, and you will have an opportunity within the time mentioned by Lord Lindsay to bring forward any propositions you choose; but to obstruct business by voting for the adjournment is not, I think, likely to be conducive to the interests of this Society.

Mr. Chambers: Mr. Dunkin has expressed surprise that this proposal should be met with opposition. I do not know what the views of others are, but my feeling on the point is this, that I very much object to any changes being choked down my throat without giving me an opportunity of considering the details. When I came into the room I was prepared to move a slight verbal amendment to paragraph No. 13, but just now, before the meeting began, I was told "that I could not do anything of the kind." I asked why, and was told "because it has been determined that we must take the alterations as they are or go without." Now, sir, this seems to me to be very unbusiness-like and inconvenient, and a very undignified way of proceeding, and I, therefore, am glad that Lord Lindsay is going to propose an adjournment of the whole subject that we may have an opportunity of studying these draft resolutions and giving notice of alterations if necessary. I was going to move the omission of the words "or fewer" in the 13th Bye-law. It is ridiculous that voting papers should be held void because they do not contain the exact number of names. I heard with great surprise the announcement, which Lord Lindsay confirmed, that we were obliged to take these amendments as we find them or go without. Under these circumstances, I urge the members who are interested in preserving their privileges to adjourn this question till November next.

Dr. Pinches: Where does Lord Lindsay find the statement that a month's notice is necessary?

Lord Lindsay: In section 10.

Dr. Pinches: "Within one month;" that is a different matter from giving a month's notice. With regard to what has fallen from these gentlemen: I happened to be concerned with Mr. Chitty in remodelling the Bye-laws of a corporate institution, and the clause to which objection has been taken was one which Mr. Chitty insisted upon, and which, I believe, will be found in the Bye-laws of almost all institutions. The object of it is to prevent the charter from becoming void.

Mr. De la Rue: It is in the present Bye-laws. There is no change.

The President: This is a point not at present before the meeting. The question is the adjournment or otherwise.

Dr. Pigott: According to the argument of Lord Lindsay, it appears to me that the meeting cannot refuse an adjournment; and, therefore, I shall certainly vote for it. The ground on which Lord Lindsay stated his case seems to me very clear. I think it is open to the meeting to controvert his arguments or the basis upon which Lord Lindsay puts his case; but no one seems to me at present to have answered his statement.

Mr. Christie: I wish to point out that the objection against considering and deciding this question at once seems to me to have very little point. If we adjourn this meeting we adjourn the consideration of the question, and we occasion fresh inconvenience, because it will be very difficult to decide it in time if these alterations are to take any effect before the annual general meeting. If any Fellow wishes to propose any amendment to these proposals of the Council it is equally open to him to propose it after they have been adopted at a special meeting (No, no), as it is at the present time. After these have been adopted he can move for another special meeting and have the matter considered. We have had a special general meeting called here, and the Fellows are all summoned together to discuss this question, and I must say it seems very absurd that they should go away without discussing it. Whether they decide one way or the other is another question, but it is a very important matter that the question should be thoroughly ventilated. Here is a proposition that we should have the discussion shirked by a simple question of adjournment. ("You cannot bring forward an amendment.") Excuse me, practically any discussion is allowable; and amendments in the way of suggestions, though they cannot be voted upon, may be brought forward. (Laughter.) If any amendment appears to the meeting to be worthy of consideration then I, for one, should not oppose the adjournment; but, I think, at any rate, we have a right to demand from those who oppose the present consideration of the propositions of the Council that the amendments proposed should be brought forward now, and that we should have the opportunity of discussing them. ("You cannot do it.") They can be proposed informally; any suggestion can be brought forward.

The President: In speaking on the proposed alteration in the Bye-laws, anyone would be in order if he made suggestions with regard to their improvement, but as he could not propose an amendment, I do not see that we should be greatly advancing the matter.

Mr. Christie: I say such amendment might be brought forward. It was my suggestion, not that they should be proposed, but that they should be stated for the consideration of the meeting, so that we might feel that we had not been called together for nothing. The Council, at any rate, have considered this matter, and if the Fellows wish to consider it further, let them say so.

Mr. Brewin: I would ask whether if you adjourn this meeting you can bring forward any question at an adjourned meeting which may not be brought forward at this meeting. Must we not dissolve this meeting altogether, and have a totally fresh meeting? Lord Lindsay has said that no amendment can be brought forward at this meeting. Is that really a fact? Bye-law 55 says no new Bye-laws or alteration or repeal of any existing Bye-law, shall be brought forward without a month's notice. That is quite true as to any new Bye-law or repeals, but does that apply to alterations to proposed alterations of alterations of the Bye-laws. I do not think that rule forbids anyone to bring forward an amendment to what is proposed. It applies to an alteration, but not to an alteration of a proposed alteration.

Lord Lindsay: Refer to the last two lines of section 10.

The President: I made enquiries very carefully before this meeting, as I felt it might possibly be my duty to decide one way or the other, if there was any difference of opinion. The best information I could gain was in accordance with my own view, that this meeting is called together to consider only the matters mentioned in the notice. Then again, it is said that for any alteration in the Bye-laws notice must be given. Notice has been given of these definite alterations, but if these are amended they become no longer the same alterations, and it appears to me that if an amendment were suffered to be made in these alterations, and voted upon at once, we should be acting hastily and possibly not legally. Therefore I was prepared to rule in the matter that no alteration should be permitted in the proposed new Bye-laws as they stand. If they are not accepted they can be negatived, and then notice can be given of any alterations, but it would be rather dangerous to permit alterations from the words of which notice has been given, for we might pass rapidly at a meeting a form of alteration which on deliberate consideration might be found to be unwise or even contradictory of another part of the Bye-laws. I think therefore that every alteration should be duly considered. These have been duly considered, but if the members do not approve of them I think it better that full notice should be given, so that alterations may be in the hands of the Fellows before they are discussed.

Mr. Snell: I do not quite agree with you, but if this ruling is accepted I have nothing to say. But I would ask whether any notice of amendment can be brought forward at an adjourned meeting.

Sir E. Beckett: I think the question put as to the effect of an adjournment is rather a serious one, but it may be easily evaded by adjourning the consideration of the question till November, and then at the November meeting we shall be able to give notice of any alteration, not a primary alteration, but amendments to the proposed alterations, and they can be discussed fully. I am quite sure it would be very inexpedient to run the risk of doing anything irregular now, because the reason of our being called here was the dissatisfaction of one or two members of the Council at the result of the last annual meeting (Hear, hear, and No, no.) It was so stated. ("No.") Everybody heard Mr. De La Rue's speech just now.

The President: I think it is hardly worth while to consider that question.

Sir E. Beckett: I am only stating what has taken place.

Mr. Brett: It was not one or two objectors, but several more. (Laughter.)

Sir E. Beckett: I am very grateful for the correction. With regard to the main point, I have waited with patience to hear whether any sort of objections would be made to the reasons given by Lord Lindsay, but I have heard none, except the extraordinary one, delivered with unusual emphasis, that this matter has been already considered by the Council. I do not think that is entirely conclusive in a meeting of this sort. I will not make any more remarks upon that except that if that is all that can be told us in favour of the amendments, it looks as if the people who urge such a reason are afraid to have the question discussed. I am sure this meeting will wish to discuss the matter without any sort of reserve, and I take the liberty of suggesting to the Council that before November, or any other time that may be fixed, they ought to take care that there shall be no difficulty in the way of amendments being proposed, because if I can see my way through a rather ambiguous phrase in the charter, I think the Council could get over the difficulty. Section 8 of the charter says, that no such general meeting shall without the consent of the Council have the power of altering or repealing any Bye-laws, unless the Fellows who may be desirous of altering or repealing such Bye-laws shall have given to the Council one month's previous notice of such intention. It strikes me, if the Council will before November formally express their consent to the whole question being open, as it would be at the February meeting, then the question may be discussed without reserve, and persons who have amendments to propose to primary alterations, will need to give notice of them. It is very unlikely that anyone should be infallible in the first instance, and therefore it is desirable we should have the opportunity of making amendments, and therefore I suggest to the Council that by their own power, or by the assistance of lawyers, they should consider the question.

Mr. Dunkin: These amended Bye-laws were drawn up by a most distinguished lawyer.

Sir E. Beckett: I dare say they were. I make that suggestion to the Council. If they find any difficulty as to it, then the conclusive end is "Let the question stand over till February." But someone says, "What will you do in the meantime?" Do as we did before. Have we not got on very well? (Cries of "No.") You had better not say no. (Laughter.) You know the reason why we have not; but I will not go into the reason, though you tempt me strongly to do so.

Mr. Brett: It would be very dreadful if you did. (Laughter.)

Sir E. Beckett: I will do so, if you like, but I do not want to provoke any angry words or discussion. All I can say is that, save one difficulty which has been pending ever since a commotion was made in this Society, three or four years ago, by Colonel Strange, Mr. Lockyer, and Mr. De La Rue, the Council have for years gone on quietly and peaceably, and if people will leave things alone, the Council will go on quietly, and we shall not be summoned to the turbulent meetings which we have had recently.

Mr. De La Rue: I rise to order, and call on Sir E. Beckett to state specifically what Messrs. De La Rue, Lockyer, and Colonel

Strange did. I insist upon its being put in a straightforward manner.

The President: I think this has nothing to do with the question. I must distinctly rule, and I hope the meeting will uphold me, that the reference of Sir E. Beckett to these three gentlemen is out of order, and has no reference whatever to the question on which he has to speak, and on this ground I think I should not be right to permit Sir E. Beckett to answer the question proposed.

Mr. De La Rue: Excuse me, as Sir E. Beckett made a statement, I ask him to complete it.

Sir E. Beckett: Then, Mr. De La Rue, I tell you that I will not. I did not want to go into the matter, but was compelled to do it by Mr. Brett's interruption.

Mr De La Rue: Then you have made a most unmanly attack.

Sir E. Beckett: You may say that if you like.

The President: The question of adjournment will soon settle itself, as the time has advanced so far that there will be none left for the consideration of the question. With reference to the point on which Sir E. Beckett has spoken, as to the Council having power to permit amendments, the Council thought it had that power, but upon consulting one or two legal gentlemen they expressed very much the opinion that Sir E. Beckett has expressed, viz., that the charter was not very clear upon the point, and therefore the Council did not feel quite sure that they would be right in acting upon the suggestion.

Mr. Bidder: I earnestly suggest to the Council the propriety of accepting Lord Lindsay's motion. Many of the alterations proposed would no doubt be improvements, but there are others which are sure to raise opposition It is impossible to believe that the Fellows would be unanimous on some of the alterations. It seems clear, however, that if we enter on the discussion tonight we must either accept the amendments as a whole or reject them as a whole, and that would be making those who voted for the alterations approve some things which they do not entirely approve, and those who rejected them object to a good deal which they might otherwise think very admirable. I think, therefore, Lord Lindsay's suggestion will be the best to adopt.

Mr. Brett: All you say would apply to any other meeting.

Mr. Bidder: I beg your pardon, you miss the point.

Mr. Ranyard: I think, Mr. President, one thing is certainly evident, that there is a considerable difference of opinion in the meeting with regard to the propriety of the proposed alterations in the Bye-laws, and it seems very desirable before we alter laws which have worked for many years satisfactorily that there should be a nearly unanimous consensus of opinion that the alterations are the best that can be made. In many societies there is a Bye-law to the effect that there must be a majority of three-fourths or four-fifths, or some large proportion of the Fellows, in order that any alteration should be made in the Bye-laws or in the constitution of the Society. We have no such Bye-law; perhaps it would be as well if we had, and, as it is evident the meeting is divided in opinion, I think it would be better to defer the consideration of the subject till November, or possibly to the February meeting (Hear, hear), then without any special leave of the Council we shall be fully in order in proposing any amendments. One other matter I should like to mention, with regard to the speech of Mr. Dunkin. I think he gave the impression just now that the Council had been quite unanimous with regard to the proposed alterations, but that was hardly the case, there was a good deal of discussion, and the present form of the proposed alterations are rather the result of a compromise. Much more sweeping changes in the Bye-laws were proposed in the beginning, but some of them were thrown out, and some of them were modified after discussion. I do not think that this method of making laws, by the compromises arrived at by the members of a committee, is the best that could be devised.

Mr. Dunkin: They were finally passed unanimously.

Mr. Ranyard: They were finally passed, but that only means that all agreed to them as a compromise. Under the circumstances, I think we had better adjourn the discussion till the February meeting. We shall not then interfere with the astronomical business of the Society at the November and December meetings. For my part, I much prefer the astronomical business of the Society to these discussions. We are obliged to undergo one general meeting in the year, but let us, if possible, keep it to one, and proceed with our legitimate business at the November and December meetings.

Mr. Snell: I consider we could proceed legally. This notice is admitted to be perfectly correct.

Mr. Bidder: Nobody disputes we could proceed legally.

Mr. Snell: The question is whether we can propose amendments.

The President: It would be hardly safe to go forward when there is a difference of opinion.

Mr. Prideaux thought if it was not open to the Fellows to move an amendment it was desirable the matter should be adjourned. The amendment to which Mr. Chambers had referred was sufficient to prove that; but whether they had the right or not to move amendments, he did not think it would be expedient to do so on the spur of the moment, and without due deliberation. He thought they ought all to have an opportunity of calmly considering the matter.

The Rev. J. C. Jackson: I think it would be advantageous to the Fellows to hear clearly how we are to have these amendments placed before us, because it would be a great gain to know how we are to present our amendments, and to know whether they will be printed and distributed.

Sir E. Beckett: There must be a month's notice.

The President: When the month's notice is given they will be printed, and will be in the hands of the Fellows, so that they will have an opportunity of considering them at their leisure.

Dr. Pinches: When they are printed it would make the matter much clearer if the present Bye-laws and the proposed alterations are printed in parallel columns.

Lord Lindsay, in reply said: Mr. Dunkin began by remarking that there was no reason for my motion for the adjournment. I think the mere discussion which has followed is sufficient justification of my action (Hear, hear), and shows that the Fellows of the Society are with me. Mr. Dunkin also said that the matter has been before the Society for the last four months. I must mention that I saw these proposed amendments some little time ago, not four months however, but that was only because I happened to be on the Council. I believe they were not published to the Society till the 26th May. That is not published to the ordinary members. Then he also said these amendments were passed by the Council without opposition. I must defend myself in this matter by saying what occurred with regard to myself. I did not offer any verbal amendment at that time, because the discussion happened to come on a day when I was wanted at the House of Commons, and I was obliged to leave the council early in the afternoon, but before going I asked the President whether the fact of my leaving without moving any amendment would deprive me of the power to do so hereafter. In fact I gave notice, like a prisoner, that I would "reserve my defence." (Laughter.) Again, Mr. De La Rue said he considered my course of action was excessively inconvenient. In answer to this I would say that I am extremely sorry it should be inconvenient. I have no doubt it is inconvenient, but I do not think the matter should be decided upon the mere question of our present convenience. And it is not as if we were met in the middle of the day, and that gentlemen had come up specially for this matter and nothing else. I think that, perhaps, then I should have greater scruples in moving for an adjournment, but as we are assembled here for the transaction of the ordinary business of the Society, I do not think there is any question of convenience at issue. Mr. De La Rue then referred to a legal opinion as to the last election, saying that these alterations were necessary in order to avoid any re-opening of a particular question; but so far as I understand the matter, certain questions were placed before several legal gentlemen, six or seven of whom decided that the action taken at the general meeting was perfectly legal. I do not see what reason there is for any alteration.

The President: One decided against and six in favour of the legality.

Lord Lindsay: It is not necessary to alter the Bye-laws because one man was doubtful as to their meaning; the legality of the action was decided by six to one. Mr. De La Rue went on to say that these motions brought forward by the Council were excessively democratic: that may be, but I should like to see them still more so. I do not wish to go into the question of the amendment, but I should wish to see them more liberal towards the body of the Fellows in general. Then I must disclaim any intention of obstructiveness. (No, no.) Mr. De La Rue used that word, and Mr. Christie also used the word "inconvenience." I think what I have said in answer to Mr. De La Rue about inconvenience would also apply to Mr. Christie's objection; but he proceeded to make rather a curious suggestion. He said, "Let us now pass the amendments as proposed, and then it will be open to any gentleman to call a general meeting and press to change them again." (Laughter.) Now that would be, I think, very inconvenient. (Laughter.) It would certainly be a waste of time, for we should have two discussions instead of one. Then Mr. Christie asked the meeting to go into a discussion of the proposals to be brought forward, and, in a manner which I can perfectly understand, demanded that the proposed amendments should be laid before the meeting at the present moment. He said my objections were ephemeral and vague, and hinted that it was doubtful whether there were any such amendments or not; but I can assure him that I will in due time bring them forward.

Mr. Christie: I did not mean to convey that there were no amendments. I merely wished to have the amendments brought forward, in order that we might be able to ventilate and discuss the question. My idea was that this special meeting should not be altogether wasted. That was the expression I used, that we might, as we are met together, discuss the question a little, whether we voted or not.

Lord Lindsay: I will satisfy your mind by saying I have amendments, but I will not propose them now. Mr. A. Brewin spoke for the adjournment, asking if a month's notice was really necessary. The 55th Bye-law speaks on that subject. It says that no business shall be brought forward except in the manner directed in the preceding section, which requires that a month's notice agreeably to the charter be given. The last two lines of the 53rd Bye-law says that no business shall be brought forward at such meeting except that which has been so notified. I think I need not say much more about that after the manner in which we have heard the President rule. Mr. Jackson proposed that the business should be adjourned according to the suggestion of Sir E. Beckett, and I shall propose to change my motion from an adjournment of the meeting to an adjournment of the consideration of the subject, and in that way the legal difficulty may be avoided.

Mr. De La Rue: Of course we must have a new special meeting.

Lord Lindsay: Yes. Mr. Bidder proposed a special meeting the week before the November meeting. It would be a great advantage I think if we could have it at the annual general meeting, because any work that is not done at the annual meeting might be adjourned without further notice. It would be far more convenient—there would be more time. Father Perry asked how amendments were to be brought forward. I imagine the course will be for any Fellow to write out his amendments and send it to one of the secretaries, and in that way it will be brought before the Council, and printed and circulated in the usual manner. Another advantage in postponing the discussion to the annual meeting is that the Council does not sit again till the November meeting, and it would be difficult to call a Council together in time to have the notices and amendments printed.

Mr. Ranyard: I must mention that five Fellows must give notice of alterations.

Lord Lindsay: Then I will move "That the consideration of this subject be adjourned until the February meeting."

Mr. De La Rue: A few words of explanation. Lord Lindsay has not understood me. What I meant to say was that it was extremely inconvenient for a member of the Council to permit a notice to call a general meeting to go out, and, when that general meeting was called, take objection to a procedure which he took part in. (Cries of "Question.")

Lord Lindsay: I think I have answered that.

Mr. De La Rue wished also to explain that he was not the originator of any of the alterations proposed, so that he was not the agitator he had been called.

After some further discussion,

The President was about to put Lord Lindsay's motion (which was seconded by Mr. Jackson) when

Mr. Christie proposed as an amendment that the consideration of the subject be adjourned to the November meeting, pointing out that it was desirable to settle the matter before the next election, as at the last election the wish of the majority had not been expressed by the result.

At the suggestion, however, of the President, it was agreed to put the question of adjournment simply, without any mention of the date of adjournment.

Mr. Bidder proposed and Mr. Christie seconded an amendment to adjourn the matter to a special meeting to be called by the Council in the last week of November.

Mr. Bidder's amendment was put, and lost by a few votes.

Lord Lindsay's proposal to resume the subject at the annual meeting in February was then put and declared by the President to be carried.

The meeting adjourned at a quarter past ten.

THE GREENWICH VISITATION.

On Saturday, the 2nd of June, 1877, we spent a very pleasant and enjoyable day in meeting the giant spirits of astronomy at the annual gathering held in the rooms and grounds of the Royal Observatory. The inspection of the instruments was a source of much gratification to the company assembled: among them we may mention the great equatorial, mainly employed in physical researches; the photoheliograph, devoted to recording solar phenomena; the alt-azimuth for improving our knowledge of the moon's motions; the chronograph for recording the time at which observations are made, and the transit-circle, the principal instrument for advancing astronomical science by determining the exact places of the sun, moon, planets, comets, and fixed stars. The magnetical and meteorological observatory, with the arrangements for photographically recording the variations of the magnetic elements, earth currents, daily temperature of the air and earth, and other collateral phenomena, was also a source of much interest. Among the novelties we noticed the sun-light indicator, for registering the duration of sun-light, the large direct-vision spectroscope, made on a new plan by Hilger, for obtaining great dispersion or great purity of spectrum by the use of half-prisms.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

THE ROYAL ASTRONOMICAL SOCIETY.

Sir,—Will you kindly permit me to make a few brief remarks on the present critical state of the Society? and I sincerely trust that they will receive the most serious consideration of the Fellows. They have been written without consultation with any other member of the Council, and therefore represent solely my own opinions on the subject.

Every one who attended the Special General Meeting on Friday last must have noticed that some of the most prominent members of the Council appear no longer to have the confidence of the majority of the Fellows who usually attend the ordinary meetings. However much this is to be deplored in the astronomical interests of the Society, nevertheless there is no doubt of the fact. Not only do differences of opinion exist, which cannot well be avoided even among the most reasonable of men, but there is also unfortunately a deep undercurrent of personal ill-feeling which has been gradually increasing since the excitement of the last annual meeting, and which all who desire to witness the advancement of our science and the prosperity of our Society must regret. The valuable services of gentlemen whose experience in practical and theoretical astronomy have naturally led them to be considered, both by home and foreign astronomers, as the backbone of the Society, appear now to have been forgotten, and there is an evident desire among a large number of the Fellows that their places in the Council should be filled by others, who must necessarily have less astronomical experience. If this wish be a genuine one, then for the well-being and orderly direction of our affairs, and to prevent in future the unseemly contests between the Council and the outside Fellows, such as we have had on several occasions lately, the composition of the Council should be so amended that it should truly represent the general body of Fellows, who alone ought to be the best judges of those whom they wish to be their representatives. The deliberate refusal to discuss the recommendations of the Council, on Friday evening last, shows distinctly what portion of the governing body has lost the confidence of the Fellows, there can, therefore, be no difficulty in pointing out who ought to retire. Some of us, probably a majority of *all* the Fellows, may regret the disappearance from the Council of many of its most active and working members, but it is an opinion which has been growing upon me for some time, that, however we may deplore the loss of the technical experience of so many distinguished astronomers, the time has come when the sacrifice should be made.

It is with great reluctance that I find it my duty to allude here to certain disrespectful charges made against those members of the Council who are also members of the Astronomical Club. These charges, which are really too contemptible to deserve notice, ~~have~~ been persistently repeated by an anonymous writer, to his great discredit, in the pages of a scientific periodical. But there is no doubt that these anonymous letters are read by a large number of our Fellows, and that they have partially assisted to bring on the present unfortunate crisis, the responsibility of which will ultimately fall chiefly on the author of these letters. When writers use the expressions contained in several of them which have lately come under my notice, as terms of reproach in reference to some of our most esteemed Fellows, they must not forget that they are casting similar reproaches on all past members of the Club, including almost every distinguished person connected with the Society who may have served on the Council. Perhaps the Fellows might wish to know the names of the present members of the Club, who are also members of the Council. I give them in alphabetical order: Professor Adams, Sir George Airy, Mr. Brett, Mr. Christie, Mr. De La Rue, Mr. Dunkin, Mr. J. W. L. Glaisher, Mr. Knott, Mr. Lassell, Lord Lindsay, and Mr. Whitbread. These gentlemen, four of whom are past Presidents, represent a majority of the Council.

The best remedy which I can suggest, to relieve the Society from the ill effects of so divided a Council, is that the whole of the above gentlemen, except Lord Lindsay, should retire in February next. and that their places should be filled up by others possessing the full confidence of the other section of the Council Thus the so-called "Club party" would vanish, and the deliberations of the Council would doubtless be carried on most harmoniously, giving no cause for unfavourable criticism on the part of the Fellows. All party feeling would then be abolished, and it is probable that the Fellows would soon learn to believe that the gentlemen who had so long governed the Society with honour have an equal interest in the promotion of our science and the prosperity of the Society as they claim for themselves. Such personal abuse, of which I have heard and read so much of late, can do no good to anybody, either to the reader and hearer, or to any writer who thinks that astronomy can be promoted by publicly imputing unworthy motives which never had an existence, except in the author's brain. As one who has always studied to avoid party feeling. having enjoyed the most friendly intercourse with every member of the Council, without exception, it is very grievous to me to find that these unfortunate personal differences have been gradually increasing in intensity till they have at last brought on a dangerous crisis in the management of the Society's affairs, the end of which it is impossible to foresee. My regrets are much increased by the thought that probably this will be the last year of my connection with the Council, where I should be sorry to sit under present circumstances. It would, however, have been more gratifying to myself to have retired while still enjoying the confidence of the Fellows, rather than as a consequence of the persistent opposition to the recommendations of the Council, which have been always considered with care and in a spirit of devotion to the interests both of the Society and the Fellows.

I have now only one concluding remark to make. It is this. The gentlemen who have promoted and brought forward the recommendations relating to the amended bye-laws, and which had been adopted by nearly every member of the Council, are those who now appear to have lost the confidence of their constituents. They have, however, the consolation of knowing that, as a labour of love, they have given their time and astronomical experience for the benefit of the Society, and that to them the great reputation which it has attained among home and foreign astronomers is mostly due.

Yours truly,
EDWIN DUNKIN.

Kenwyn, Blackheath:
1877, June 11.

THE COMING OPPOSITION OF MARS.

Sir,—I forward for the use of any of your subscribers who take an interest in this planet, and desire to make drawings of its surface, some prepared forms, which I hope will prove acceptable.

The ground of the paper is covered with a tint very similar to the general tone of the planet; upon this the dark marks may be drawn with pencil, and the stump employed to soften in the usual manner. The lights are produced with a sharp penknife, and may be erased of any required strength, from the faintest tone to pure white for the Polar snows.

I am, sir, yours very respectfully,

3, Circus Road,
St. John's Wood.

NATHL. E. GREEN.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN JULY, 1877.

By W. R. Birt, F.R.A.S., F.M.S.

The Mare Crisium, Eastern Border.—The Rev. W. J. B. Richards calls attention to the four objects north of the eastern "Pass," marked by Schröter *l*, *m*, *n*, p. (See Lunar Objects for May, 1877, *ante* p. 125). These objects have been carefully compared with Schröter's drawings and the following results obtained:—Taking *l* (I C *π* 8, or for colloquial purposes Peirce 8) as an angular point, *m* (Peirce 11) and *Prom: Olivium* (I B *ν* 3) as the western and eastern legs of a triangle; Schröter and Richards perfectly agree in the angle *m*, *l*, *P*: again taking *l*, *m* as the western and *l*, *n* as the eastern leg of a second triangle, Schröter and Richards also agree in the angle *m*, *l*, *n*. This identification of Schröter's settles any question that may be raised as to the existence of these three objects. Schröter gives in eight drawings *l*, *n*, and *p* in a line as stated in the list for May. Richards gives in his drawings *p*, west of the line joining *l* and *n*, and questions whether the object which he has taken for *p* be really Schröter's *p*. The agreement among Schröter's eight drawings combined with the identity of his and Richard's angles speaks greatly in favour of his accuracy.

The late fine weather has afforded us an opportunity not only of verifying and confirming the positions of Schröter's *p* as seen by Richard's, but of *recovering* the crater *μ* to which we have alluded in our May list, where we have marked it I C λ 20; it is, however, in area I B°, of which it is I B° 9. We have three observations of it; on June 14, "visible, but not easy because of the surrounding mountains;—June 15, easier than last night, it is situated in a plain surrounded by mountains; June 16, seen, but not so distinctly as last night. This crater forms the apex of a triangle of which Picard and I C λ 3 form the western leg, and I B° 10, I B° 11 the eastern. The angle Picard, I B° 9, I B° 11 at the time of the observations was less than a right angle.

Errata in May list, p. 125, line 7, for I C λ 20, read I B° 9; line 10, for I C λ 17, read I B° 10, for I C λ 18, read I B° 11, for I C λ 20, read I B° 9, and in the two following make similar corrections.

Hawkenbury, Buckhurst Hill:
June 20, 1877.

ASTRONOMICAL OCCURRENCES FOR JULY, 1877.

DATE.			Principal Occurrences.	Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Jupiter.
Sun	1	0 18	Conjunction of Moon and Mars 5° 33′ S. Conjunction of Moon and Jupiter 4° 4′ S.	2nd Tr. I. 2nd Sh. I. 2nd Tr. E. 2nd Sh. E.	10 14 10 49 13 1 13 37	— 11 7·6
Mon	2		Sidereal Time at Mean Noon, 6h. 42m. 30·95s.			11 3·1
Tues	3	9 1	☾ Moon's Last Quarter Sun's Meridian Passage 3m. 55·31s. after Mean Noon			10 58·7
Wed	4			1st Tr. I. 1st Sh. I.	14 18 14 40	10 54·2
Thur	5	13 54	Near approach of B.A.C. 782 (6½)	1st Oc. D. 1st Ec. R.	11 38 14 14 56	10 49·8
Fri	6			1st Tr. I. 1st Sh. I. 1st Tr. E. 1st Sh. E.	8 45 9 9 11 0 11 25	10 45·3
Sat	7			1st Ec. R. 3rd Oc. D.	8 43 33 12 10	10 40·9
Sun	8			2nd Tr. I. 2nd Sh. I.	12 30 13 25	10 36·5
Mon	9	14	Conjunction of Moon and Mercury 4° 3′ S. Saturn's Ring: Major axis=40″·94 Minor axis=0″·50			10 32·1
Tues	10	10 6	● New Moon	2nd Ec. R.	10 19 39	10 27·7
Wed	11	16	Conjunction of Moon and Venus 0° 42′ S.			10 23·3
Thur	12	3	Conjunction of Mars and ψ^2 Aquarii 0° 1′ S.	1st Oc. D.	13 23	10 18·9
Fri	13	8 19 9 5 9 26 9 34	Occultation of B.A.C. 3538 (6½) Reappearance of ditto Occultation of 44 Leonis (6) Occultation of B.A.C. 3562 (6½)	1st Tr. I. 1st Sh. I. 1st Tr. E 1st Sh. E.	10 30 11 4 12 45 13 20	10 14·5
Sat	14	9 17	Occultation of 75 Leonis (5½)	1st Ec. R.	10 28 15	10 10·1
Sun	15		Illuminated portion of disc of Venus=0·945 Illuminated portion of disc of Mars=0·907			10 5·7

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Jupiter.
Mon	16		Sidereal Time at Mean Noon 7h. 37m. 42·77s.			10 1·4
Tues	17	1 12	☽ Moon's First Quarter	**2nd Oc. D.** **2nd Ec. R.**	8 52 12 55 43	9 57·0
Wed	18		Sun's Meridian Passage 5m. 56·86s. after Mean Time	**3rd Sh. E.**	10 36	9 52·7
Thur	19	17	Superior conjunction of Mercury and Sun	**2nd Sh. E.**	8 7	9 48·4
Fri	20	11 51	Occultation of τ Scorpii (3½)	**1st Tr. I.** **1st Sh. I.**	12 16 12 58	9 44·1
Sat	21			**1st Oc. D.** **1st Ec. R.**	9 25 12 33 2	9 39·7
Sun	22	8 39	Near approach of B.A.C. 6220 (6½)	**1st Tr. E.** **1st Sh. E.**	8 58 9 43	9 35·4
Mon	23					9 31·2
Tues	24	19 19	○ Full Moon	**2nd Oc. D.**	11 11	9 26·9
Wed	25	13 30	Near approach of η Capricorni (5½)	**3rd Tr. I.** **3rd Tr. E.** **3rd Sh. I.**	8 32 11 17 11 45	9 22·6
Thur	26			**2nd Tr. E.** **2nd Sh. E.**	9 3 10 43	9 18·4
Fri	27	9	Conjunction of Saturn and Mars 3° 43′ S.			9 14·1
Sat	28	23 23	Conjunction of Moon and Saturn, 4° 17′ S. Conjunction of Moon and Mars 8° 6′ S.	**1st Oc. D.**	11 22	9 9·9
Sun	29		Saturn's Ring; Major axis=42″·16 Minor axis=0″·77	**1st Tr. I.** **1st Sh. I.** **1st Tr. E.** **1st Sh. E.**	8 29 9 22 10 45 11 38	9 5·7
Mon	30	4	Neptune at quadrature with the Sun	**1st Ec. R.**	8 56 37	9 1·4
Tues	31					8 57·2
AUG. Wed	1			**3rd Tr. I.**	12 1	8 53·1

THE PLANETS FOR JULY.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	5 26 15	N.21 56	6″·3	
	9th	6 30 28	N.23 35	5″·4	
	17th	7 44 9	N.22 46	5″·0	
	25th	8 46 58	N.19 51½	5″·0	
Venus ...	1st	7 47 37	N.22 27½	10″·1	
	9th	8 22 58	N.20 32½	10″·2	
	17th	9 9 3	N.18 0	10″·4	
	25th	9 47 48	N.14 57	10″·6	
Mars ...	1st	22 58 22	S. 11 8	18″·8	
	9th	23 9 32	S. 10 26½	20″·4	
	17th	23 18 27	S. 9 58	22″·0	
	25th	23 24 47	S. 9 45	23″·6	
Jupiter ...	1st	17 47 59	S. 23 10	43″·5	
	9th	17 43 56	S 23 9½	43″·1	
	17th	17 40 20	S. 23 9	42″·6	
	25th	17 37 20	S. 23 9	42″·0	
Saturn ...	1st	23 27 45	S. 5 42	16″·2	
	9th	23 27 34	S. 5 45½	16″·6	
	17th	23 26 60	S. 5 51½	16″·6	
	25th	23 26 3	S. 5 59½	16″·8	

Mercury may be observed in the morning, rising about an hour and ten minutes before the sun on the 1st, the interval decreasing. Towards the end of the month he sets after the sun.

Venus is visible for an hour after sunset throughout the month.

Mars rises about an hour before midnight, at the beginning of the month, the interval increasing.

Jupiter may be seen till three hours after midnight on the 1st, the interval decreasing to about one hour at the end of the month.

Saturn rises about three quarters of an hour before midnight on the 1st, and then gradually earlier each night.

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF JUPITER.

Greenwich Noon.	Angle of position of ♃'s axis.	Longitude of ♃'s central meridian.	diff.	Latitude of Earth above ♃'s equator.	Equatorial diameter.	Defect of illumination.
1877.	°	°		°	″	″
July 1	0·40	289·7	870·7	—2·27	45·99	0·02
2	0·45	80·4	·7		45·95	·02
3	0·51	231·1	·7		45·92	·03
4	0·56	21·8	·7		45·88	·03
5	0·62	172·5	·7		45·84	·04
6	0·67	323·2	·7		45·79	·04
7	0·73	113·9	·7		45·74	·04
—						
8	0·78	264·6	870·7	—2·26	45·69	0·05
9	0·83	55·3	·7		45·64	·06
10	0·88	206·0	·6		45·58	·06

11	0·93	356·6	·7		45·52	·07
12	0·98	147·3	·7		45·46	·07
13	1·03	298·0	·7		45·39	·08
14	1·08	88·7	·6		45·33	·08
—						
15	1·13	239·3	870·7	—2·26	45·26	0·09
16	1·18	30·0	·7		45·18	·10
17	1·22	180·7	·6		45·11	·10
18	1·27	331·3	·7		45·03	·11
19	1·31	122·0	·6		44·95	·12
20	1·35	272·6	·7		44·87	·12
21	1·39	63·3	·6		44·79	·13
—						
22	1·43	213·9	870·7	—2·24	44·70	0·14
23	1·47	4·6	·6		44·61	·14
24	1·51	155·2	·6		44·52	·15
25	1·55	305·8	·7		44·43	·16
26	1·58	96·5	·6		44·34	·16
27	1·62	247·1	·6		44·24	·17
28	1·65	37·7	·6		44·14	·18
—						
29	1·68	188·3	870·6	—2·23	44·04	0·18
30	1·71	338·9	·6		43·94	·19
31	1·74	129·5	·6		43·84	·20
Aug. 1	1·77	280·1			43·74	·21

Defect of illumination on following side.

A. M.

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF MARS.

Greenwich Noon.	Areographical longitude of the centre of ♂'s disc.	‖	latitude	Angle of position of southern pole.	Diameter of disc.	Amount and position of greatest defect of illumination.	
1877.	°	°	°	°	"	"	°
July 1	58·6	350·5	—24·35	161·81	15·70	1·81	249·6
2	49·1	·5	24·33	161·57	15·85	1·81	
3	39·6	·5	24·30	161·32	16·01	1·81	249·7
4	30·1	·5	24·27	161·08	16·17	1·81	
5	20·6	·5	24·24	160·85	16·33	1·80	249·8
6	10·1	·5	24·21	160·63	16·49	1·79	
7	1·6	·6	24·18	160·41	16·65	1·78	249·9
—							
8	352·2	350·5	—24·15	160·20	16·82	1·78	
9	342·7	·5	24·11	159·99	16·97	1·77	250.1
10	333·2	·6	24·07	159·79	17·14	1·76	
11	323·8	·6	24·03	159·60	17·31	1·75	250·2
12	314·4	·6	23·99	159·41	17·48	1·74	
13	305·0	·6	23·95	159·23	17·65	1·72	250·4
14	295·6	·6	23·91	159·06	17·82	1·71	
—							
15	286·2	350·6	23·86	158·89	17·99	1·69	250·6
16	276·8	·6	23·82	158·73	18.16	1·67	
17	267·4	.7	23·77	158·57	18·34	1·65	250·9
18	258·1	·6	23·73	158·43	18·51	1·63	
19	248·7	·7	23·68	158·29	18·69	1·61	251·1
20	239·4	·7	23·63	158·16	18·87	1·59	

[illegible]

[illegible]

[illegible]

[illegible]

[illegible]

[illegible]

[illegible]

[illegible]

[illegible]

[illegible]

[illegible]

[illegible]

	21	230·1	·7	23·58	158·03	19·05	1·57	251·4
	—							
	22	220·8	350·7	—23·54	157·91	19·23	1·55	
	23	211·5	·7	23·49	157·80	19·41	1·52	251·7
	24	202·2	·7	23·44	157·70	19·59	1·49	
	25	192·9	·7	23·39	157·60	19·77	1·46	252·1
	26	186·6	·8	23·34	157·51	19·95	1·43	
	27	174·4	·8	23·29	157·43	20·14	1·40	252·5
	28	165·2	·8	23·24	157·36	20·32	1·37	
	—							
	29	156·0	350·8	23·19	157·29	20·50	1·34	252·9
	30	146·8	·8	23·14	157·23	20·69	1·31	
	31	137·6	·8	23·10	157·18	20·87	1·27	253·4
Aug.	1	128·4		23·05	157·14	21·05	1·23	

A. M.

THE MINOR PLANET PALLAS.

The minor planet Pallas, No. 2 of the series, will come into opposition on the 13th of the month.

	R. A. h.	m.	s.	Decl. °	′	″
July 1	19	14	53·43	+21	31	12
9	19	8	20·62	+21	8	11
13	19	5	4·70	+20	50	9

Erratum in No. 174, page 146, line 13 from bottom, for *face* read *force*.

Books Received.—"Report of the Astronomer-Royal to the Board of Visitors of the Royal Observatory, Greenwich, 1877."—"Meteorological Observations made at the Adelaide Observatory by C. Todd, C.M.G., F.R.A.S. Adelaide: W. Cox. 1877."—"British Rainfall, 1876, compiled by G. J. Symons. London: Edward Stanford, Charing Cross, S.W. 1877."

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To December, 1877.

Fleming, Rev. D.
Franks, W. S.
Watson, J.

TO CORRESPONDENTS.

We cannot publish communications which are not authenticated by the name and address of the sender, as a guarantee of good faith.

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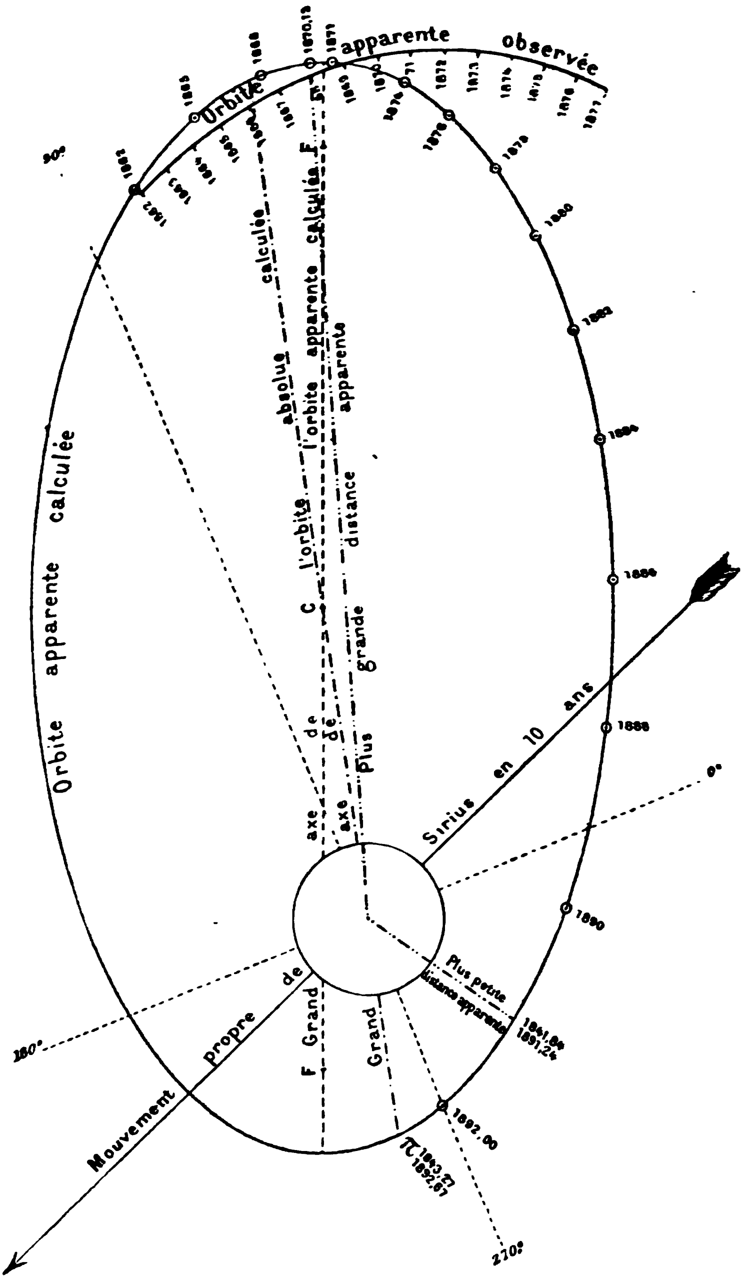
The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. Jackson, *Clarence Road, Clapton, E.*, **not later than the 15th of the Month.**

ORBIT OF THE COMPANION TO SIRIUS.



By Camille Flammarion.

The Astronomical Register.

No. 176. AUGUST. 1877.

THE ROYAL ASTRONOMICAL SOCIETY.

No one can regret more than we do that there should be serious disagreements among the Fellows of the Royal Astronomical Society, but we cannot for a moment doubt that in so serious a question as that of altering the Bye-laws of one of the oldest of the so-called learned societies, it was both undignified and unwise that there should have been even the appearance of haste. It seems strange that it was expected by the most sanguine and intrepid of the proposers of the strong measure laid before the last meeting, that it was in any way possible to pass it in an hour or so; in other words, without discussion. There seems to have been a curious exhibition of impatience on the part of some members of the Council, which was hardly justified by the merits of the case. Of course, if the Council of the Astronomical Society is to be considered the Society itself, and if the Fellows are to be so many puppets to nod their heads as the Council bids them, and to pass without consideration or discussion what has seemed good to the "Patres conscripti," then of course they merited the ire of some of those who have had to do with these proposed alterations. But if the general body of Fellows has any respect for itself, it must have a real voice in so serious a matter concerning the well-being of the Society. There cannot be the slightest question in our minds that the Royal Astronomical Society should never have been called upon to pass sweeping alterations in their Bye-laws without being able to propose even verbal alterations. The whole affair bears on its face too much the appearance of the Council trying to ride the high horse over the other Fellows. "Take them all or you shall not have them at all; we have decided that these alterations are absolutely necessary, and that the form in which we propose them is the best which could possibly be made." Now this seems to us a very unwise and very unfair way of treating the subject. There are many who doubt the wisdom of any change, and there

is much to be said on their side. These might of course have voted without discussion against the alterations, but there are probably still more who think that some change, if not necessary, is still likely to conduce to the advantage of the Society. It was simply absurd to suppose that, if they could help it, the Fellows would consent to pass alterations in which they had no voice, unless they entirely agreed with them.

If the Council are placed in an unpleasant position, it is surely their own fault. It must have been a most unbusinesslike proceeding even to attempt to force these resolutions, the result only of a compromise, upon the general body of Fellows. It was unbusinesslike, because it was unfair, and we may add foolish; for however well the subject might have been considered, surely it was most unwise to preclude the possibility of making the slightest even verbal alteration. The Council must consider themselves to be very wise indeed, if in a matter, not of astronomical, but of ordinary business, they did not expect to gain any advantage from the advice of one single Fellow not a member of the Council. In matters of pure astronomy we are ready to admit that the ordinary Fellows are not always competent to form an opinion. If this were not so we should not, as we occasionally do, excite the astonishment of outsiders at some of the gentlemen we elect as our Council, and at our rejection of others of whom any Society out of England might be proud. This however is merely a matter of transacting business, and there are very many Fellows with only a general interest in matters astronomical, who are far more competent to give a valuable opinion upon it, than the most careful and admirable of observers or the best mathematicians and computers of the body. We should have thought that the Council would not only have been willing, but have desired, that their acts and propositions should be discussed by the Fellows in the fullest and freest way possible.

Lord Lindsay showed simply very good sense in moving that the consideration of the question should be deferred to a time when it might be fully and thoroughly discussed. If he had not done so, we happen to know that there are others who would; but the proposal could not have fallen into better hands. We shall be glad to receive communications from the Fellows upon the subject.

THE COMPANION OF SIRIUS.

By Camille Flammarion.

The observations made on the companion of Sirius, discovered in 1862, are now rather numerous and extend over an arc extensive enough to allow us to examine whether this companion follows strictly the orbit obtained by theory.

It will be remembered that the periodical irregularities observed in the proper motion of Sirius, even as far back as 1844, led Bessel to suppose that there was in the vicinity of this brilliant star an invisible disturbing body, to whose influence these irregularities might be attributed. They especially are remarkable in right ascension: the difference between the mean right ascension and the actual right ascension according to observations has been from + 0,058s. in 1842, and of — 0,133s. in 1850. In 1851 Peters, following the hypothesis of Bessel, calculated the theoretical orbit which would agree with the observed perturbations, and found:

Passage through the inferior apsis . .	1791·431
Mean annual motion	7°, 1865.
Period	50 years, 01.
Eccentricity	0,7994.

Eleven years later, on the 31st of January, 1862, Alvan Clarke found out the companion as he turned upon Sirius his new objective of 47 centimetres. The satellite was plunged in the rays of the brilliant star, but, nevertheless, was easily perceptible. It is a star of ninth mag. Since then it has been carefully observed every year.

It was to be found almost exactly at the east, and its position agreed pretty well with the theoretical position. In 1864 Auwers made a new calculation of the orbit according to the whole of the observations of the proper motion, and found the following elements, which do not differ much from the first, and confirm them:

Passing by the inferior apsis . .	1793 890.
Mean annual motion	7°,28475.
Period	49 years, 418.
Eccentricity.	0,6010.

The last orbit calculated by Auwers, put in the form of orbits of double stars (see *Nature* of 30th March, 1876), and offered as definitive, is the following one:

$$\tau = 1843{,}275$$
$$\Omega = 61^\circ\ 57'{\cdot}8$$
$$\lambda = 18\ \ 54{\cdot}9$$
$$\iota = 47\ \ 8{\cdot}7$$
$$e = 0{\cdot}6148$$
$$a = 2''{\cdot}331$$
$$P = 49 \text{ years } {\cdot}399$$

According to these elements the limits of distance should have been 2″,31, at 302°, 5, in 1841, 84 and 11″. 23, at 71°,7 in 1870, 13 and the ephemeris is—

	°	″
1862·0	85·4	10·10
1865·0	79·9	10·78
1868·0	75·0	11·15
1871·0	70·3	11·20
1874·0	65·5	10·95
1876·0	62.1	10·59
1878·0	58·4	10·05
1880·0	54·2	9·33

Now, here is the series of micrometrical measurements which I have collected and inserted in my *Catalogue of double stars in motion.*

1862,08	85±	10±	AC.
62,19	84,6	10,07	Bo.
62,20	85,0	10,09	Ru.
62,28	83,9	n. m.	Ls.
63,15	88,4:	7,63:	Se.
63,21	81,2	9,54	Ru.
63,21	82,5	10,15	Σ_2.
63,27	82,8	n.m.	Bo.
64,08	78,5	10,50	Mr.
64,15	80,3	9,53	Ls.
64,15	79,6	10,90	Mr.
64,20	79,2	10,40	*id.*
64,21	80,1	9.67	Ls.
64,22	78,6	10.70	Bo.
64,22	76,5	10,92	Σ_2.
64,23	84,9	10,00	Da.
64,24	79,7	n.m.	Wi.
65,20	77,2	10,60	Σ_2.
65,20	76,8	10,77	Fo.
65,21	77,2	10,60	Σ_2.
65,22	75,0	10,07	Se.
65.24	77,8	10,77	Fo.
65,25	76,8	n.m.	Ti.
65,26	76,0	9,0	Bo.
65.30	77,0	9,0	En.
66,07	77,2	10.43	Kn.
66,20	n.m.	10,74	Bh.
66,20	75,2	10,93	Σ_2.
66 20	74,1	11,29	Fo.
66.20	73,8	10,97	Ti.
66,22	75,4	10.57	Ea.
66,22	74,0	10,20	Ha.
66,23	74,9	10,57	Nw.
1866,24	78,6	10,34	Tu.
66,26	75,4	10,57	Ea.
66,29	71,3	10,11	Se.
67,22	72,1	10,98	Σ_2.
67,24	72,3	n.m.	Fo.
67,28	74,5	9,80	Ea.
68,23	70,2	11,25	Vo.
68,24	69,5	11,35	Bh.
68,26	71,6	10,95	En.
69,10	74,7	10,28	Br.
69,15	73,5	11,23	Vo.
69,20	68,6	11,26	Du.
70,24	65,0	12,06	Vo.
71,23	64,0	11,21	Du.
71,25	60,1	12,10	Pe.
72,18	59,8	11,14	Du.
72,25	62,7	11,55	Nw.
73,20	65,8	11,12	Ha.
73,22	60,9	10,65	Du.
73,91	59,4	12,27	Ha.
73,93	64,9	11,29	Ws.
74,16	59,0	11,46	Nw.
74,19	58,7	10,99	Ho.
74,23	57,9	11,10	Ha.
75,19	57,1	10,81	Du.
75,21	56,6	11,41	Nw.
75,21	55.9	11,89	Ho.
75,28	56,3	11,08	Ha.
76,09	54,9	11,82	Ho.
76,21	55,2	11,19	Ha.
77,16	52,8	11,39	Ho.
77,29	53,4	10,95	Ha.

AC., Alvan Clark; Bo., Bond; Ru., Rutherfurd; Ls., Lassell; Se., Secchi; Σ_2, O. Struve; Mr., Marth; Da., Dawes; Wi., Winnecke; Fo.,

Förster; **Ti.**, **Tietjen**; **En.**, **Engelmann**; **Kn.**, **Knott**; **Bh.**, **Bruhns**; **Ea.**, **Eatsman**; **Ha.**, **Hall**; **Nw.**, **Newcomb**; **Tu.**, **Tuttle**; **Vo.**, **Vogel**; **Br.**, **Brunnow**; **Du.**, **Dunér**; **Pe.**, **Peters**; **Ws.**, **Wilson & Seabroke**; **Ho.**, **Holden.**

If we compare the observations with the ephemeris we see that the angle diminishes more quickly than it had been announced, while the distance has kept on increasing from 1870 instead of having reached the maximum in that year, as was indicated by the calculations of the orbit. This is very easily proved by a single glance at the *drawing* which I have made, on the exaggerated scale of one centimetre for one second. The observed apparent orbit crosses from 1869 the calculated apparent orbit, and projects outside, following a quite different course which will be more extended and less eccentric. The observed companion certainly belongs to Sirius, and does not form with the brilliant star a mere group perspective; for, if it were so, Sirius would have gone from it in the direction indicated by the arrow, which represents its proper motion (A.R. —0″503 [—0s.,035 × 15 × cos D.]; D.P. + 1″,23; result out = 1″,33). The mean angular motion is only 2°·15 and can be considered as having been regular,* taking into consideration the rather sensible errors of observation in this pair so difficult of measurement. If that were the *mean* general motion, the revolution of the satellite would be much longer than it ought to be (totally) to agree with disturbers, it would be increased to about 167 years. But the arc passed over is still too small for us to decide anything on that point, and as the irregularities of the proper motion require the period to be 49 years, we are led to conclude, either that the observed companion will accelerate its motion and will be found at the west in 1892, or that there is another disturbing body not yet discovered, which is nearer and more rapid.

We must reserve any conclusion on the existence of one or more satellites as well as on any difference of period between the observed orbit and the calculated one; but the undeniable fact to be deduced is, that the observed positions do not agree with those of the ephemeris, and that the orbit which results differs from that calculated.

THE GERMAN ASTROPHYSICAL OBSERVATORY NEAR POTSDAM.

This observatory is being erected at some distance to the south of the town of Potsdam, on a wooded eminence, which is called the Telegraphenberg (Telegraph Hill). There could hardly be imagined a better spot for an observatory. In the first place the elevated situation allows a free view of the whole sky, and in the second place the reflection of the heat from the ground is all round essentially lessened by the woods which cover it; a circumstance which becomes of considerable influence with regard to the calm of the surrounding strata of the air. The fenced-off area, most readily placed at the disposal of science, is of enormous size, representing an extent of surface of 179,000 square metres, so that in this manner also for the future the too close vicinity of non-scientific buildings would be avoided. The latter contingency, however, is not at all to be apprehended. The railway station of the Berlin-Potsdam line is easily reached from the observatory in fifteen minutes, so that communication with Berlin is greatly facilitated. The spot affords that seclusion necessary for scientific labours without being subject to the unpleasant incidents of an isolation from intercourse.

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* Yet we notice a rather striking tardation from 1866 to 1869, and an acceleration from 1869 to 1871.

As regards the *Buildings*, there are at present finished four dwelling houses, the machinery house, together with gas factory and the well, whilst the chief observatory is still in building. Three of the dwelling houses are destined for the observers and assistants, and the fourth, situated at the entrance, for the under-officials of the institute.

The *Well*, which has a depth of 46 metres, is an important part of the whole design; two steam-engines, by means of an extensive network of pipes, supply both the buildings and garden-grounds with the clear water of this well. It has been constructed in a very roomy way, and stairs of easy passage lead to the level of the water. At a depth of 25 metres the shaft of the well has a connection with a vaulted place at its side, which is intended for such scientific researches which must be made at a temperature as constant as possible, and which require freedom from concussion. Out of this subterranean chamber two earthenware pipings lead to the surface of the ground. In the wall of the well there are left at certain distances openings for the reception of thermometers, in order to make continuous observations of the temperature of the earth.

The gas establishment produces for the lighting of dwelling rooms, laboratories, etc., the so-called Fettgas (fatty gas) which is obtained from the residue resulting from the manufacture of tar.

With regard to the *Observatory* itself, it is expected that its erection will be finished in the course of the summer of 1877, and that probably in the autumn of 1878 the premises will be in a fit state for being occupied, at which time then it will also be possible to begin the observations more extensively. The ground plan of this building has the T form. At the point where the two wings intersect is the chief tower, with a room for observations of 10 metres in diameter. It is destined for the reception of a refractor with an aperture of 12 inches English, which, in order to make the best use of the available space, will be placed on a vaulted pillar. To the east and west of it there are two more towers for observation being erected, which, by means of colonnades, are brought in connection with the rest of the building, so that a protected communication between all the parts of the observatory will be possible. Before the chief tower, towards the south, but of course in a somewhat lower situation, a room is being prepared, destined for the reception of a heliograph for the purpose of completing the observations of the sun by continuous photographic registrations of them.

That wing of the building situated in the direction from north to south, which partly encloses the chief tower, contains on both sides of a corridor, in two stories, the physical, chemical, and photographic laboratories, the working rooms, the conference and library rooms as well as residence, and workshop for a mechanician, who is to be engaged.

Towards the north the building is closed by a tastefully-built water tower, which at the same time will be made use of for the placing of some necessary meteorological instruments.

On the roof of the building there will be erected a photographic glass-house in order to be able to photograph original drawings and other scientific objects. The flat areas of the roof left free are to receive a covering of turf on account of its lessening the reflection of heat.

To effect the warming of the building in winter an apparatus for producing heated air will be provided, the chimneys of which will be to the north of the towers for observation, and at the greatest possible distance from them. As regards the dimensions of the building, everything has been arranged in a spacious manner, without the mistake of too great diffuseness having been fallen into. The whole arrangement may be designated as practical, and fully answering the purpose for which the observatory is intended.

The *Instruments* are being obtained from various working places; H. Schröder, of Hamburg, for instance, has undertaken to furnish the optical part of the large refractor, whilst A. Repsold & Sons, of the same place, are going to carry out the parallactic mounting. Schröder has already finished the objective, which will very shortly be tried and taken over by a scientific commission; he furnishes besides a finder of one metre's focal distance, the necessary eye-pieces, a polarizing helioscope. The tube for the telescope also has been constructed according to his designs. This tube is, for the sake of greater lightness, made of wood, and is at the end of the eye-pieces so arranged that the various auxiliary apparatus, such as spectroscopes, etc. may easily be put on. A large spectral-apparatus with seven Rutherfurd's prisms, which some time ago was produced at Schröder's works, can likewise be attached to this telescope. The mechanical part of this instrument will be carried out with the application of the most recent inventions and improvements, and the name of Repsold is a guarantee for the solid execution of the work.

The second largest telescope with a free objective 8-inches aperture (English), is being furnished by Grubb, of Dublin, and according to latest intelligence, already finished. This instrument will be put up in the western flank-tower.

The third telescope is already in existence and in use. It is a smaller refractor of 5-inches aperture, with clockwork, circles, etc., and serves for the fixing of the position of sun-spots, and for observations of protuberances. It is as yet placed in a little wooden house, but will afterwards be put up in the eastern flank-tower.

The instrument for photographing the sun—*Heliograph*—will probably also be constructed by Schröder. Its tube is to be fixed in a position parallel with the axis of the earth, and the image of the sun is to be reflected into it by means of a mirror. The achromatism of the objective, according to the statement of the reporter, will be shown only for those rays which have an effect on Iod- and Bromsilver, having particular regard to the different intensity of the effect. A trial-objective, arranged according to these principles, has, in photographing fine linear systems at a great distance, given the most favourable results. The ascertaining of position will be effected with the above-mentioned instrument by means of the level.

Of *Clocks,* the Institute possesses already four. One pendulum-clock, with temperature and air-pressure compensation, by Th. Knoblich, in Hamburg. This clock, to judge by the trials made at the Berlin Observatory, goes eminently well. (It was exhibited at the Loan Exhibition.)

The remaining three are box chronometers by Kessels, Knoblich, and Tiede.

For terrestrial spectral-researches, photographic and other work, there have already existed for some length of time instruments, apparatus, and other expedients. Lately there has been procured in addition a large Geissler's quicksilver air-pump.

The *labours* to be carried on will probably be of various kinds.

The observations of the heavens will comprise spectral-analytical researches of the sun and the stars, observations of nebulous stars and double stars, also researches of the physical nature of the planets, and the making of drawings of them; likewise photometric and especially spectral-photometric labours, registration of sun-spots and protuberances, (the former by means of photography) and other accessory labours.

Besides these celestial researches, there will also be terrestrial ones carried on, which relate to the physical development of the heavenly bodies, there will therefore be the most modern and best apparatus procured in order to obtain powerful effects of light and heat.

In conclusion, a few words about the scientific staff, and the direction of the Institute. There is a committee composed of three persons, the members of which live already within the precincts of the observatory, and who will later on conduct the practical carrying out of the observations and researches. They are the following gentlemen: Dr. Vogel, Professor Spörer, and the author of this report. The authority next above these consists again of three members and goes by the title of "Direction." It is composed of the following gentlemen: Professors Auwers, Förster and Kirchhoff, and is sitting in Berlin.

This organisation must be considered a very fortunate arrangement; it protects the development of the Institute from the one-sidedness which, when the Direction is in the hands of one person only, often occurs with regard to the perception of the opportunity for certain researches, and it necessarily engenders an impartial and many-sided consideration concerning any measures to be taken.

Thus it seems admissible to presume that everything required has been done to ensure a favourable development for this new abode of natural science.

Potsdam: June, 1877. (Signed)
Dr. OSW. LOHSE.

GIOVANNI SANTINI.

(From the *Astronomische Nachrichten.*)

At one o'clock on the 26th inst., died at his villa, Padua, Prof. Giovanni Santini, at the great age of over 90 years. Born at Caprera in the Province of Arezzo, Jan. 30th, 1787, he studied astronomy at Pisa with Piazzini, and at Milan under Oriani and Cesaris. In Nov. 1806, he was appointed Associate Astronomer, and in 1813 ordinary Professor of Astronomy, and Director of this Observatory. . . . This is not the place, nor would it be possible in a few lines, to give a detailed account of the scientific activity of Santini, an idea of it may be formed by considering that he was one of the most assiduous observers and calculators, and that his scientific life embraces the period of time between 1807 and 1870. To him are owing very numerous observations of comets, planets, occultations, eclipses, many computations of orbits, several stellar catalogues, a number of very valuable memoirs scattered in the proceedings of noted academies, and two masterly works—"The Elements of Astronomy," and "The Theory of Optical Instruments." The readers of the *Astronomische Nachrichten* have a specimen of his activity in the many contributions sent by him to this journal, and contained in its first 60 volumes. Besides his scientific merits Santini deserves to be remembered for the services rendered by him in the instruction of youth as teacher and director in the Faculty of Mathematics in this University, and for his civil and domestic virtues, which made him universally beloved and respected. In the Observatory of Padua, which owes to him its chief lustre, and which preserves so many documents of his unwearied laboriousness, Santini leaves a memory cherished and venerated.—From the Astronomical Observatory of Padua. 28th June, 1877.

G. LORENZONI, Associate-Astronomer.

FUNERAL OF SANTINI.

On Wednesday [June 27] the last honours were rendered to the eminent astronomer and philosopher Santini. There were present at the funeral the Prefect of Padua, the municipal authorities, the members of the University, the local authorities, and Prof. Ferrai. Prof. Legnazzi read over the bier the *éloge* of the distinguished man of science. The various scientific institutions were also represented, and scientific notabilities attended.—Padua, June 30th (*From an Italian Paper*).

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

THE ROYAL ASTRONOMICAL SOCIETY.

Sir,—As an old member of the Council of the Royal Astronomical Society, whom the "Club-party" most strenuously tried (and failed) to expel from that Council in February last, I must crave permission to reply to Mr. Dunkin's very remarkable letter in this month's *Astronomical Register*. Like him, "I have written without consulting any other member of the Council," and, of course, merely utter "*my* own opinions on the subject."

And, in the outset, a word as to this said "Club-party," the reference to which by Mr. Dunkin is certainly more ingenious than ingenuous. Heaven forbid that I should class every member of the Astronomical Club as belonging to the clique which—following him—I have so designated. I would confine it simply to the compact little party which essays to rule the Society from Willis's Rooms. It is merely an insult to the common sense of the Fellows at large, to include Sir George Airy, Mr. Knott, Mr. Lassell, or Lord Lindsay among that party. Assuredly there are no four names upon our list which shed more lustre or confer more dignity upon the Society than theirs, and it would be a most serious misfortune if any one of them were to join in Mr. Dunkin's cry of sour-grapes and retire. It is the gentlemen who, on December 8, 1876, at their tavern table, openly boasted of and laughed at the "coup d'état," by which Captain Abney's name had just been substituted for Mr. Ranyard's in the House-list as Secretary, to whom I refer. Mr. Dunkin will surely recollect who they were. To mix them up with the four (at least) whose names I have quoted above, is simply an attempt to throw dust in the eyes of the Fellows generally. Perhaps, however, Mr. D. will tell us whether Captain Abney is a member of the Club? We all know that Mr. Ranyard is not, and it is equally notorious that I have nothing, proximately or remotely, to do with it.

But now let us endeavour to discover what Mr. Dunkin means when he says that "some of the most prominent members of the Council appear no longer to have the confidence of the majority of the Fellows who usually attend the ordinary meetings."

A little plot is hatched in December to replace that faithful servant of the Society, and admirable Secretary, Mr. Ranyard, by Captain Abney. In what way "the astronomical interests of the Society" were to be advanced by such substitution, no one has, so far, condescended to inform us. Mr. Ranyard is a sound mathematician, has worked most indefatigably for us in a variety of ways, and (as I happen to know personally) is familiar with a vast mass of detail in several branches of astronomy. Captain Abney, on the other hand, is an amiable and courteous gentleman, a most eminent photographer—*et præterea nihil*—but I have yet to learn that these are sufficient qualifications, in any sense, for the Secretaryship of a purely *astronomical* society. However this may be though, the little plot, as I have said, was hatched, and the little mine sprung in December last, and by the narrowest majority, Mr. Ranyard's name was excised from the Secretaryship in the House-list. At the same time, advantage was taken of a thin meeting of the Council, to exclude my name altogether, and so exuberant was the delight of a great friend of Mr. Dunkin's at the success of this last "coup" that (before the result of the ballot could possibly have been known to any human being outside our own body) he actually set "an anonymous writer, to his great discredit, in the pages of a scientific periodical," to crow (perhaps a little prematurely) over the "retirement" of "the last of the objectionable members from the Council." *Nemo me impune lacessit*, and the Annual Meeting gave *him* the lie. It did something more. It restored Mr. Ranyard to a position from which an utterly mean and contemptible attempt had been made to oust him, and it was within an ace of sending one or two of the most conspicuous of the authors of the "coup d'état" themselves to the right-about altogether. At this (to them) wholly unexpected result, the party who had so thoroughly made up their minds that they were the Society were dumbfoundered; but, beaten in one way in their efforts to confine all its offices to their personal friends, their first effort was to upset the election altogether. Its hopeless collapse is now a matter of history, so no further reference is needed to it here. Failing this, then, the next thing was to get the Bye-laws altered. (In what follows I must entreat every Fellow who is interested in the subject to check my statements seriatim, by reference to the minutes of Council, which he is entitled to peruse at Burlington House). Well, then, after some discussion it was suggested to refer the whole matter to a Committee. Whereupon three of the men who were most urgent that the Bye-laws should be changed were appointed as such Committee, and when I remonstrated, ventured to point out that *both* sides of the question should be represented upon it, if any real inquiry were to be made, and proposed to add the name of Captain Tupman, as that of an absolutely impartial and unprejudiced member, I was clamorously put down; no one, by the way, making more noise than Mr. Dunkin himself. Merely as a matter of course, and as of a foregone conclusion, this one-sided and prejudiced Committee reported to the Council at large that the Bye-laws ought to be altered, and some very remarkable alterations indeed were suggested, more than one of which would have subjected the Fellows wholly and solely to the dictation of the Council. As the result, however, of a protracted discussion, a sort of compromise was effected, and the suggested alterations, as laid before the June meeting, were passed by a large majority. I did not myself vote either for or against them, and I fancy, but am not sure, that I was not singular in this. If we are to assume that our existing Bye laws need alteration at all (which I deny) then I may say that with one or two of the suggested changes I, in the main, agree; one or two I regard as of the most doubtful expediency, while I absolutely and entirely disapprove of those relating to the audit. It however, never entered into my mind to conceive that these cut-and-dried resolutions would be presented to the Special Meeting, with "take them or leave them, there they are," or without the very fullest opportunity of discussing them, and moving amendments to them seriatim being afforded to the Fellows generally. The meeting decided (and, as I think, righteously and properly decided) that it would not have organic changes introduced into the Bye-laws of the Society without ample consideration; merely because certain members of the Council were anxious about their friends' seats in February, 1878. The "confidence" (the loss of which Mr. Dunkin so feelingly deplores) would then I presume have been shown by the meeting opening their mouths and shutting their eyes, and taking what he and his friends condescended to send them.

The plain truth is that the whole of this attempted sensational legislation was *pro re nata*—and the meeting knew it. Does anybody—does Mr. Dunkin himself—believe for a single instant that, had Captain Abney replaced Mr. Ranyard, and I been turned out of the Council altogether, one syllable would ever have been heard about altering the Bye-laws? He *knows* there would not; and it is just because the Fellows at large share this knowledge that they are determined to have every suggested change categorically discussed.

I do not think it necessary to refer to Mr. Dunkin's "puff oblique" of those who act with him in the Council, further than to ask him to what extent "the experience in practical and theoretical astronomy" of certainly one of those allies exceeds that of the waiter who pours out our coffee after the evening meetings? The gentleman referred to may be identified by the perfect taste of his persistent interruptions at the Special Meeting in June. I am really curious to know the source of the anxiety of Mr. Dunkin's friends to retain this colleague of theirs upon the Council. If he "be considered, both by home and foreign astronomers, as" any part of "the backbone of the Society," it must surely be as one of the caudal vertebræ! We know of course that he dines, and holds up his hand to vote, like Mr. Christie's shadow; but one hardly likes to think that it can be for any such vulgar and commonplace reasons as these that such solicitude is exhibited to retain him among those who "have the consolation of knowing that, as a labour of love, they have given their time and astronomical experience for the benefit of the Society, and that to them the great reputation which it has attained among home and foreign astronomers is mostly due."

I do not know whether Mr. Dunkin read his own concluding sentence over again after penning it, but if he did it can scarcely have failed to strike him that, in that modest and unassuming little peroration, he distinctly implies that those who did *not* promote and bring "forward the recommendations relating to the amended Bye-laws," can only have served the Society (if at all) for their own private and personal advantage; and that the "labour of love" is strictly a monopoly of himself, and the few other gentlemen to whom (as he asserts) the Society owes its "great reputation."

I have the honour to be, Sir,
Your most obedient servant,
WILLIAM NOBLE.

Forest Lodge, Maresfield,
Uckfield: July 5, 1877.

Sir,—I cannot but regret that my friend, Mr. Dunkin, should have written to you a letter so little calculated to restore peace to the Astronomical Society, or even to maintain what remains of former good fellowship, as that which appears in the *Register* for this month. It is manifest that his purpose is excellent; but, as I judge, it is equally manifest that he fails to recognise the real posture of affairs or the fitting remedy for the troubles at present affecting the Society.

In the first place, exception must be taken to the distinction which he draws between the Council (which he calls "the governing body") and those whom he calls the "outside Fellows." The Council, I take it, is not appointed to govern the Society, but to manage its affairs; and Fellows are elected to serve on the Council—that is, to serve the body corporate, consisting of *all* the Fellows.

But passing over the unfortunately dictatorial tone which Mr. Dunkin, unintentionally perhaps, assumes, I would call his attention to the fact that as yet nothing has happened which should render the restoration of peace impossible; whereas, if the views which he advances were just, peace could hardly be restored without changes which would to all intents and purposes ruin the Society. He says, "there is no doubt of the fact, that the most prominent members of the" present "Council have lost the confidence of the majority of Fellows who usually attend the ordinary meetings," and later on, he leaves us in no doubt as to the members to whom he refers; for he gives a list of names, including those of Prof. Adams, Sir George Airy, Mr. De La Rue, and Mr Lassell. Now, though I have not attended closely to the events which have taken place in the Society of late, I am certain if there had been the slightest movement, in any section of the Society, implying want of confidence in Mr. Lassell, Prof. Adams, or Sir George Airy, I and others, now equally ignorant of any such movement, *must* have heard of it. I believe that if the question were put to the vote of the whole Society whether any one of those members should no longer remain upon the Council, *not one single vote* would be given in favour of a proposition so outrageous. With regard to Mr. De La Rue, the case is only different for one reason;

he has, unfortunately, allowed his name to be used in support of schemes of whose propriety he has no doubt satisfied himself, but which are looked upon with grave suspicion by a large section of the scientific world. This, I take it, explains the fact, for such it is, that he no longer receives, as of yore he used to receive, the unquestioning and unanimous support of the Society.

I do not refer to the other names mentioned by Mr. Dunkin, because I know very little about the views of most of the Fellows he refers to, on that subject about which *alone* the Society is divided,—the question, namely, whether the weight of the Astronomical Society should be thrown into the scale in favour of the establishment of a physical observatory, or laboratory, or generally for the endowment of scientific research.

The question of the medal was too paltry to disturb the peace of the Society. It was raised in Col. Strange's now historical circular, or rather in his explanation of that circular; but no one knew better than Strange that he and Messrs. De La Rue and Lockyer had left the Council,—he himself writing a letter to the President assigning as his reason the rejection of the observatory scheme—several months before the circumstances happened which he subsequently assigned as the occasion of his and their dissatisfaction.

Mr. Dunkin knows so well what really was the *fons et origo malorum*, that he can scarcely be in earnest in his account of what happened at the last general meeting and at the meeting last June after it was made special. He must be aware that if two names had appeared in the House-List issued by the Council last December, no change would have been called for; and he knows also that, without issuing any circular or making any very special efforts to obtain votes, the friends of the two members in question succeeded in securing their election. If the issue of a circular signed by Sir G. Airy, Prof. Adams and Mr. De La Rue, before the general meeting, had implied that the two former desired the removal from the Council of the two Fellows above referred to, there might have been some reason for Mr. Dunkin's suggestion that Sir G. Airy and Prof. Adams are not in accord with the majority of the Society. But I happen to know that one at least of the two Fellows who with Mr. De La Rue signed the circular did so with no other object than the very proper one of securing a large attendance of Fellows.

There was nothing whatever in the modification which the House-List received which should seriously or permanently affect the peace, and therefore the well-being, of the Society.

As to the proceedings at the meeting last June, they amount, on the face of them, to nothing more than the assertion of a wish on the part of the majority of those present to have time to examine the modifications of the Bye-laws suggested for their consideration. It is certain from what actually transpired that, had the question been put to the vote—Shall these modifications, as they stand, be passed or not?—the Noes would have had it. This would have been unfortunate, because little doubt exists that some of the modifications are very desirable. The Society can exist, very well, I conceive, till February, 1879, when such modifications as are carried next February will come into operation. In the meantime there will be opportunity, not only to study the proposed changes, and others which may perhaps be suggested, but also to secure arrangements which will meet the approval of the whole Society, or at least a very large proportion of it.

All that Mr. Dunkin says about charges made by an anonymous writer in the pages of a scientific periodical seem to me to be very much beside the question. It is true that those charges glance in some degree at himself, at least he has been invited to contradict rumours which, if believed, would affect him. (For my own part I should regard any man as very much my friend who called my attention, however publicly, in fact the more publicly the better, to rumours injuriously affecting myself, till contradicted.) If Mr. Dunkin had contented himself with such contradictions instead of using somewhat unpleasant expressions respecting the writer referred to (and, by the way, omitting in his warmth to avail himself of the opportunity offered of setting himself right), his friends would have been pleased. I think I know the writer of the letters referred to, and if I am right in my surmise, I can assure Mr. Dunkin that the gentleman referred to has long had a hearty esteem and liking for himself personally and also (which is perhaps more to the point) is thoroughly persuaded that the schemes he has opposed really exist, and really deserve all that he has said against them. I may remark also that Mr. Dunkin makes a common mistake, but a mistake all the same, if he supposes, as he seems to do, that discredit attaches to anonymous writing in periodicals, scientific or otherwise. I do not myself write much anonymously, except when I have no choice, as in leader-writing, and so forth, and even then I generally manage to indicate my identity by some statement or turn of expression. But I protest on the part of those who do so write, against the supposition that an anonymous writer who is known by name to the editor, under whose sanction his remarks appear, is open to the discredit justly due to the writer of an anonymous letter properly so called. The writer of an anonymous letter is necessarily a coward, and probably a liar, and as a rule creates more mischief than ordinary liars. But the list of anonymous writers in periodicals includes the names of many whom the world has agreed in esteeming very highly indeed.

All the same, I think it is to some degree unfortunate that the anonymous writer referred to, entertaining as he does the most thorough conviction of the justice of his charges, and of the impropriety of the schemes for which the approval of the Astronomical Society has lately been anxiously sought, has not called on the Society as a whole to pronounce its verdict upon them. If this were done the matter might probably be disposed of finally and forgotten, after which perhaps our ordinary meetings might resume the pleasant character which formerly distinguished them, and many Fellows might again attend who are at present deterred from so doing by these foolish contentions.

R. A. PROCTOR.

TABLES OF LUNAR LIBRATION.

Dear Sir,—On page 251 of your tenth volume I find the following paragraph:

"The monthly tables for sun and moon are computed with the help of auxiliary tables which may be published at some convenient occasion before long. The formulæ for libration and others will then probably be found preferable to those hitherto known."

Is not the present a "convenient occasion" for the publication of these tables and formulæ? There must be many of your readers who, now that you no longer give the monthly tables of libration, would be glad to have these auxiliary tables and formulæ which they might find useful in calculating the libration for themselves.

I am, dear Sir, yours faithfully,

July 22, 1877. W. J. B. RICHARDS.

Dear Sir,—Thanks for the communication of Mr. Richards' note. Is your correspondent not rather fanciful? *How many* of your readers, I wonder, would really take the trouble of calculating the libration for themselves? What use is made of the tables for computing libration which are given in the *Nautical Almanac?* And how many readers have taken advantage of the facilities provided for them some years ago, when regular ephemerides for physical observations of the moon were printed in the *Astronomical Register?*

It happens that during the next lunations the libration-curves are of an uncommonly curious character. I will give below the daily librations from to-night to the end of the next lunation, and if there are any readers, who are interested in the subject, they will, perhaps, not mind the trifling trouble of laying down these librations as rectangular co-ordinates on squared paper, and of constructing the libration-curves for themselves. The scale ought to be sufficiently large, say, about an inch to a degree.

On July 29, Aug. 7, Aug. 25, Sept. 4, the moon's libration is such that the circularity of the preceding limb will be affected by some depressions of considerable depth, about which something may be found in the *Astronomical Register* for Dec. '75, and in the last *Monthly Notices* for June, '77. These depressions will not be observable on the nights mentioned, the preceding limb being in darkness. But their existence, as well as that of any considerable elevations, will have to be taken into account, whenever necessary, and independently of the demands of selenography, the outline of the moon's rim will have to be studied in all states of libration, if, for instance, the results of observed star occultations are to be kept free from avoidable errors due to considerable irregularities of the rim. The point of the moon's surface, where the star disappears or reappears, must then be strictly computed for each observation, etc. But the suggestion must suffice here.

66, Lambeth Road, S.E.
Yours faithfully,
A. MARTH.

GEOCENTRIC LIBRATION.

Greenwich midnight. 1877.	in long. λ_0	in lat. β_0
	°	°
July 24	+2·36	+3·71
25	+1·05	+2·47
26	—0·34	+1·12
27	1·76	—0·27
28	3·16	1·66
—		
29	—4·49	—2·98
30	5·66	4·19
31	6·62	5·23
Aug. 1	7·28	6·05
2	7·57	6·59
3	7·44	6·80
4	6·87	6·64
—		
5	—5·84	—6·07
6	4·41	5·10
7	2·67	3·77
8	—0·74	—2·16
9	+1·24	—0·38
10	3·10	+1·41
11	4·76	3·07
—		
12	+6·05	+4·50
13	6·97	5·62
14	7·48	6·37
15	7·59	6·76
	°	°
Aug. 16	+7·34	+6·80
17	6·76	6·50
18	5·92	5·91
—		
19	+4·86	+5·06
20	3·64	4·00
21	2·31	2·78
22	+0·92	1·44
23	—0·49	+0·02
24	1·88	—1·37
25	3·20	2·72
—		
26	—4·40	—3·96
27	5·45	5·05
28	6·29	5·91
29	6·88	6·51
30	7·16	6·80
31	7·10	6·75
Sept. 1	6·67	6·32
—		
2	—5·85	—5·52
3	4·65	4·34
4	3·13	2·86
5	—1·37	—1·16
6	+0·53	+0·64
7	+2·42	2.39

A. M.

D'ARREST'S COMET.

Sir,—Many of your readers will be interested to learn that D'Arrest's periodical comet has again become visible. It was detected by M. Coggia at Marseilles, on July 9, by the aid of Leveau's ephemeris, so much of which as relates to August I beg leave to subjoin.

	R. A. h. m. s.	Decl. ° ′ ″
August 2	3 46 2	+6 35 38
4	3 49 59	6 29 25
6	3 53 50	6 22 32
8	3 57 34	6 15 0
10	4 1 12	6 6 49
12	4 4 43	5 58 0
14	4 8 7	5 48 35
16	4 11 24	5 38 34
18	4 14 34	5 27 58
20	4 17 38	5 16 49
22	4 20 34	5 5 6
24	4 23 23	4 52 51
26	4 26 4	4 40 5
28	4 28 38	4 26 49
30	4 31 4	+4 13 2

The comet is described as very faint. It is approaching both the earth and the sun, but during the whole of its apparition will need for its examination telescopes of considerable size.

Northfield Observatory, I am, Sir, your obedient servant,
Eastbourne: July 27, 1877. GEORGE F. CHAMBERS.

LINNÉ.

Sir,—In the *Astronomical Register* for May, I see that Mr. Thomas W. Backhouse, of Sunderland, takes exception to a statement of mine on page 7 of the *Quarterly Journal of Science*, for June, 1877. I there state that "between 1867 and 1869 several observers considered that they could detect traces of the ring of a very shallow crater around the crater cone in the position of Linné, and in size slightly larger than the crater of Beer and Mädler. The actual existence of this ring is doubtful: it has not been seen since 1869, and the appearance noticed probably arose from a number of ridges and mounds near the position of Linné."

In his letter to the *Astronomical Register*, however, Mr. Backhouse denies that the faint ring has not been seen since 1869; he having seen it "in 1873, 1874, 1875 and 1876 with a 4½-inch reflector;" and he describes it as being "not a very difficult object" although it is "not visible for many hours at each time." But as Mr. Backhouse goes on to say that what he saw was, perhaps, not the faint ring of the shallow crater at all, but only something which looked like it in his telescope, I do not see on what ground he questions the accuracy of my statement. Especially as my statement could only fairly be held to apply to published observations.

I have frequently examined Linné to see if I could detect the supposed shallow crater, employing telescopes of considerably greater power than that used by Mr. Backhouse, but I have never been able to see it. Several of our best known selenographers have also looked for this object but with a like result, and, since 1869, Mr. Backhouse is, I believe, the only observer who claims to have seen this formation. There are mounds and ridges near Linné which observers inexperienced in lunar work might take for the ring of a shallow crater, but they could not well deceive practical observers using instruments of adequate power. As Mr. Backhouse is, I believe, not deficient in experience in observing the moon, I can only suppose that the telescope employed by him is not sufficiently powerful to show these very delicate objects in their true form. These mounds and ridges are certainly not only delicate, but downright difficult objects, and seldom clearly visible, so that if they are easy objects to Mr. Backhouse his eyesight must be most remarkably keen.

Captain Noble seems to have misunderstood the purport of Mr. Backhouse's letter, because his communication to the *Astronomical Register* for June refers to an entirely different point to the one in question. It is not disputed that Linné at times appears like a minute crater cone with a black interior, I have seen it so myself on many occasions. But this crater cone is of very minute dimensions, having a diameter of only two miles, and a crater-like opening of two-thirds of this size. Mr. Backhouse referred to the large shallow crater which was once supposed to exist, and to have a diameter of from seven to nine miles ; a very different object to the small crater cone seen by Captain Noble. There are few lunar observers, too, who can like Captain Noble see Linné "distinctly and beautifully as a ring or crater" with a telescope of only little more than 4-inches aperture.

Whilst referring to Linné, I would direct the attention of observers to a minute hill north-east of Linné, and distant about two miles from the base of the wall. It is a very delicate object indeed, but last year I had reason for suspecting that it was a crater cone. It should be watched. Another object requiring attention is a lofty peak ε, on the west of the deep valley Cleomedes κ, which cuts the mountain border to the Mare Crisium on the north-east. It is an 8° bright mountain, nearly 9,000 feet high, with, I believe, a crater cone on the summit nearly 9° bright.

E. NEISON.

June 20, 1877.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN AUGUST, 1877.

By W. R. Birt, F.R.A.S., F.M.S.

Although selenography has made good progress of late, and some selected portions of the moon's surface have undergone close scrutiny, much remains to be done ; not only do we need for the general purposes of selenography accurate positions of conspicuous spots, but a more or less continuous history of each, thus our study may be divided into two parts, mathematical and physical. Our brief monthly notices have reference to the second, and we should like to see some determined efforts for extending its culture ; could not some plan be devised by which the cultivators of physical selenography may be able to combine and concentrate the results of their labours, and thus stamp a value on their observations which they lack at present?

We are certainly approaching a state of lunar research in which greater attention will be bestowed on the smaller features of the moon's disc, indeed the latest observations are of this character, and when we remember that a space equal to one second of arc represents *a mile on the earth's* surface, if we desire to learn any thing of the *true* nature of the moon's surface we must study its minute detail. Several observers have of late closely *watched* the smaller objects on Fracastorius, the Mare Crisium and other formations, and we are on the eve of obtaining the commencement of a history of certain objects in both the Mare

Crisium and Fracastorius. On the eastern part of the Mare Crisium, as is well known, are three conspicuous craters, viz., Picard, Peirce and Peirce A, or I C π 2, the surface of the Mare immediately surrounding Picard and Peirce is of a dark brownish or purplish colour, it is the cause or causes of their alternations of light and dark both as regards time, as referred to the luni-solar day, and space as regards the moon's surface, that are the subjects of elucidation.

Mr. Herbert Sadler has kindly furnished the following observations of the three craters mentioned above, which we recommend not only for observation in August, but as often as observers conveniently can:—

May 24, 1877, 9.20 to 10.5, aperture 6½-in., powers 164, 80 Definition fluttering, moon low.

Picard, "White spot." Two diameters of Picard W.E. to the W. of Picard, ½ to ⅓ size of I C λ 3, slightly larger than I C ν 3, in form pear-shaped, length 1⅔ times its greatest breadth, moderately bright, but not so bright as Peirce A, not quite so bright as I C ν 3, markedly less bright than Linné and less bright than the walls of Bessel or Bessel A (I E ε 1), ± 3°·75 as a whole ½°—⅓ less bright than the E. sides of Picard and Peirce. A very small spot not 1″ across about the centre of the pear-shaped aureole. This speck' not over 4¼° bright, the aureole 3¼°—3½°.

The dark portion of the Mare round Picard evident, the boundary between the lighter and darker portion of the Mare to the W. of Picard passes just to the W. of the white spot, and is evidenty the ridge which is so conspicuous soon after sunrise. Picard is to the E. of the oval-shaped darker portion of the Mare.

Peirce, surrounded by a dark patch similar to, but smaller than, that round Picard; a minute white speck, 3¼° to 3¾° bright, is to the W. of Peirce in the dark portion, and is like a minute craterlet, under a high sun, it is about ⅓ the diameter of Peirce, W.E. from the west wall of Peirce.

Peirce A. Can hardly be distinguished, probably a small oval white marking ¾ size of I C λ 3 in centre of a darkish space, similar to those round Picard and Peirce. This darkish space is bounded on the south by a light streak from Proclus which separates it from a dark portion round Peirce; a whitish spot is west of the line joining the centres of Peirce and Peirce A (?), in the light streak, and the white spot I have seen before to the N. of Peirce A. is evident to night.

The *Monthly Notices* of the R. A. S., June, contains an identification of the Rev. Cooper Key's flattenings on the moon's western limb, by Dr. Copeland, of Dunecht.

Buckhurst Hill:
July 19, 1877.

TOTAL ECLIPSE OF THE MOON.

August 23, 1877. Visible at Greenwich.	G. M. T. h.	m.
First contact with the penumbra, Aug. 23	8	3·7
First contact with the shadow	9	13·7
Beginning of total phase	10	19·1
Middle of eclipse	11	11·4
End of total phase	12	3·7
Last contact with the shadow	13	9·1
Last contact with the penumbra	14	19·1

Magnitude of the eclipse (Moon's diameter=1), 1·682.

The first contact with the shadow occurs at 59° from the northernmost point of the moon's limb towards the east. The last contact 112° towards the west—in each case for *direct* image.

ASTRONOMICAL OCCURRENCES FOR AUGUST, 1877.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Altair.
Wed	1	 22 20	Sidereal Time at Mean Noon, 8h. 40m. 47·69s. ☾ Moon's Last Quarter	3rd Tr. I.	12 1	— 11 2·2
Thur	2	10 35	Occultation reappearance of 2 Arietis (4½)	2nd Tr. I. 2nd Sh. I. 2nd Tr. E.	8 36 10 30 11 24	10 58·3
Fri	3		Sun's Meridian Passage 5m. 53·82s. after Mean Time			10 54·3
Sat	4					10 50·4
Sun	5			3rd Ec. R. 1st Tr. I. 1st Sh. I.	8 39 24 10 17 11 17	10 46·5
Mon	6			1st Ec. R.	10 51 32	10 42·5
Tues	7			1st Sh. E.	8 2	10 38·6
Wed	8	17 17	● New Moon Eclipse of the Sun invisible at Greenwich			10 34·7
Thur	9			2nd Tr. I.	10 59	10 30·7
Fri	10	4 15	Conjunction of Moon and Mercury 1° 27′ N. Conjunction of Moon and Venus 2° 49′ N.			10 26·8
Sat	11			2nd Ec. R.	10 4 11	10 22·9
Sun	12			3rd Oc. R. 3rd Ec. D.	8 16 9 55 32	10 18·9
Mon	13			1st Oc. D.	9 26	10 15·0
Tues	14			1st Sh. I. 1st Tr. E 1st Sh. E.	7 41 8 50 9 57	10 11·1
Wed	15	10 27	☽ Moon's First Quarter Illuminated portion of disc of Venus=0·886 Illuminated portion of disc of Mars=0·975			10 7·1
Thur	16		Sidereal Time at Mean Noon 9h. 39m. 56·03s.			10 3·2

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Altair. —
Fri	17	11 21	Sun's Meridian Passage 3m. 49·02s. after Mean Noon Conjunction of Uranus and Sun Conjunction of Moon and Jupiter 5° 2′ N.			9 59·3
Sat	18		Saturn's Ring: Major axis=43″·04 Minor axis=1″·23	2nd Oc. D.	7 34	9 55·3
Sun	19	11 19	Near approach of τ Sagittarii (3½)	3rd Oc. D.	9 8	9 51·4
Mon	20			2nd Sh. E. 1st Oc. D.	7 49 11 17	9 47·5
Tues	21			1st Tr. I. 1st Sh. I. 1st Tr. E.	8 25 9 36 10 41	9 43·5
Wed	22			1st Ec. R.	9 10 14	9 39·6
Thur	23	11 10	○ Full Moon Eclipse of the Moon visible at Greenwich			9 35·7
Fri	24					9 31·7
Sat	25	16 1 1 2 22	Near approach of 21 Piscium (6) Conjunction of Moon and Saturn, 4° 14′ S. (6) Conjunction of Moon and Mars 8° 46′ S. Conjunction of Saturn and Mars 4° 32′ S.	3rd Oc. D.	10 3	9 27·8
Sun	26					9 23·9
Mon	27			2nd Sh. I. 2nd Tr. E. 2nd Sh. E.	7 35 7 53 10 25	9 19·9
Tues	28			1st Tr. I.	10 17	9 16·0
Wed	29	8 27 9 17	Occultation of μ Arietis (5½) Reappearance of ditto	1st Oc. D.	7 36	9 12·1
Thur	30	12 43 13 39 12 53 13 24	Occultation of 19 Tauri (5) Reappearance of ditto Near approach of 20 Tauri (5) Near approach of 16 Tauri (5½)	1st Tr. E. 3rd Sh. I. 1st Sh. E. 3rd Sh. E.	7 1 7 44 8 17 10 41	9 8·1
Fri	31	9 15	☾ Moon's Last Quarter			9 4·2
SEPT. Sat	1	10 0 10 15 16 38 17 26	Occultation of B.A.C. 1746 (6½) Reappearance of ditto Occultation of 136 Tauri (3) Reappearance of ditto			9 0·3

THE PLANETS FOR AUGUST.

At Transit over the Meridian of Greenwich.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	9 40 8	N.15 37½	5″·2	0 59·2
	9th	10 32 34	N.10 5	5″·4	1 20·0
	17th	11 16 35	N. 4 27	6″·0	1 32·5
	25th	11 53 16	S. 0 48½	6″·6	1 37·6
Venus ...	1st	10 20 41	N.11 55½	10″·8	1 39·6
	9th	10 57 14	N. 8 10	11″·2	1 44·6
	17th	11 32 58	N. 4 11	11″·4	1 48·8
	25th	12 8 12	N. 0 4½	11″·8	1 52·5
Mars ...	1st	23 27 54	S. 9 47½	25″·2	14 44·7
	9th	23 28 23	S. 10 5½	26″·8	14 13·7
	17th	23 24 20	S. 10 47	28″·5	13 39·4
	25th	23 19 46	S. 11 17½	29″·1	13 2·2
Jupiter ...	1st	17 35 18	S. 23 9	41″·4	8 53·1
	9th	17 33 43	S. 23 9½	40″·4	8 20·6
	17th	17 32 59	S. 23 10	39″·6	7 47·8
	25th	17 33 7	S. 23 11½	38″·8	7 16·5
Saturn ...	1st	23 24 57	S. 6 8½	16″·9	14 41·7
	9th	23 23 23	S. 6 20	17″·0	14 8·7
	17th	23 21 34	S. 6 33	17″·2	13 35·4
	25th	23 19 32	S. 6 47	17″·3	13 2·0

Mercury sets from 40 minutes to half-an-hour after the sun during the month.

Venus sets about an hour after the sun on the 1st, the interval very slightly decreasing.

Mars rises an hour and a half after sunset at the beginning of the month, the interval decreasing.

Jupiter may be seen till about an hour after midnight on the 1st, after which he sets earlier each night.

Saturn rises rather more than hour after sunset, at the beginning of the month, the interval decreasing.

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF MARS.

Greenwich Noon. 1877.	Areographical longitude of the centre of ♂'s disc.	Areographical latitude of the centre of ♂'s disc.	Angle of position of southern pole.	Diameter of disc.	Amount and position of greatest defect of illumination.	
	°	°	°	°	″	″	°
Aug. 1	128·4	350·8	—23·05	157·14	21·05	1·23	
2	119·2	·9	23·01	157·10	21·23	1·19	253·9
3	110·1	·8	22·97	157·07	21·41	1·15	
4	100·9	·9	22·92	157·05	21·58	1·11	254·5
5	91·8	350·9	—22·88	157·04	21·76	1·07	
6	82·7	0·9	22·84	157·04	21·93	1·03	255·2
7	73·6	1·0	22·80	157·05	22·10	0·99	
8	64·6	0·9	22·76	157·06	22·27	0·94	256·0
9	55·5	1·0	22·73	157·08	22·44	0·90	
10	46·5	0·9	22·70	157·11	22·60	0·86	256·8
11	37·4	1·0	22·66	157·15	22·76	0·82	

12	28·4	351.0	—22·63	157·19	22·91	0·77	257·8
13	19·4	·0	22·60	157·24	23·06	0·73	
14	10·4	·1	22·58	157·30	23·21	0·68	258·9
15	1.5	·0	22·55	157·37	23·35	0·64	
16	352·5	·0	22·52	157·44	23·49	0·59	260·3
17	343·5	·1	22·50	157·52	23·62	0·55	
18	334·6	·1	22·48	157·61	23·75	0·50	261·8
—							
19	325·7	351·1	—22·46	157·71	23·87	0·46	
20	316·8	·1	22·44	157·81	23·98	0·42	263·7
21	307·9	·1	22·43	157·92	24·09	0·38	
22	299·0	·1	22·42	158·04	24·19	0·34	266·0
23	290·1	·1	22·41	158·16	24·28	0·30	
24	281·2	·2	22·40	158·29	24·37	0·27	268·9
25	272·4	·1	22·39	158·42	24·45	0·24	
—							
26	263·5	351·2	—22·39	158·56	24·52	0.21	272·7
27	254·7	.1	22·39	158·70	24·58	0·18	
28	245·8	.2	22·40	158·85	24·63	0·15	277·8
29	237·0	·2	22·40	159·00	24·67	0·12	
30	228·2	·1	22·41	159·16	24·71	0·10	284·9
31	219.3	·2	22·42	159·32	24·74	0·08	
Sept. 1	210·5		—22·43	159·48	24·76	0·07	295·2

In Vol. XI., p. 166 of the *Astronomical Register*, a doubt is expressed respecting the dates of some of the sketches of Mr. Banks and Mr. Green, published in Vol. III., p. 71. An error of the month in Mr. Green's sketch No. 3, of 1864, has been corrected in Vol. XI., p. 181. It now turns out that a similar error has been committed in the date of Mr. Bank's sketch, No. 6, of 1864. In consequence, the line

67·9 —9·5 15·8 Banks 6* 64 Dec. 17 10 20

in the list of sketches on page 153 of the present volume, must be struck out, and must be replaced on page 154 by the line

332·6 —3·5 17·4 Banks 6* 64 Nov. 17 10 20

A. M.

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF JUPITER.

Greenwich Noon. 1877.	Angle of position of ♃'s axis.	Longitude of ♃'s central meridian.	diff.	Latitude of Earth above ♃'s equator.	Equatorial diameter.	Defect of illumination.
	°	°		°	″	″
Aug. 1	1·77	280·1	870·7	—2·22	43·74	0·20
2	1·80	70·8	·6		43·63	·21
3	1·82	221·4	·6		43·53	·22
4	1·84	12.0	·6		43·42	·23
—						
5	1·87	162·6	870·5	—2·22	43·31	0·23
6	1·89	313·1	·6		43·20	·24
7	1·91	103·7	·6		43·09	·25
8	1·93	254·3	·6		42·97	·25
9	1·95	44·9	·5		42·86	·26
10	1·96	195·4	·6		42·75	·26
11	1·98	346·0	·6		42·63	·27
—						
12	1·99	136·6	870·5	—2·20	42·52	0·27
13	2·00	287·1	.6		42·40	·28
14	2·01	77·7	·5		42·28	·29

15	2·02	228·2	·6		42·16	·29
16	2·02	18·8	·5		42·04	·30
17	2·03	169·3	·6		41·92	·30
18	2·03	319·9	·5		41·80	·31
—						
19	2·03	110·4	870·6	—2·19	41·68	0·31
20	2·03	261·0	·5		41·56	·31
21	2·03	51·5	·5		41·44	·32
22	2·03	202·0	·5		41·32	·32
23	2·03	352·5	·6		41·20	·33
24	2·03	143·1	·5		41·08	·33
25	2·02	293·6	·5		40·96	·33
—						
26	2·01	84·1	870·5	—2·17	40·83	0·34
27	2·00	234·6	·5		40·71	·34
28	1·99	25·1	·5		40·59	·34
29	1·98	175·6	·5		40·47	·35
30	1·96	326·1	·5		40·35	·35
31	1·95	116·6	·5		40·22	·35
Sept. 1	1·93	267·1			40·10	·35

A. M.

MINOR PLANET CERES.

The minor planet Ceres, the first of the series, will come into opposition on the first of the month.

	Right Ascension.			Declination.		
	h.	m.	s.	°	′	″
July 18	21	13	52·83	S.28	7	58
„ 26	21	7	18·44	S.29	0	5
Aug. 1	21	1	58·89	S·29	35	33

Books received.—"The Unseen Universe," by B. Stewart and P. G. Tait. London: Macmillan & Co. 1876.—"Les Merveilles Célestes," par Camille Flammarion. Paris: Hachette & Co. 1875.—"Hints on Silvered Glass Reflecting Telescopes." By G. Calver. 1877.—"On Possibility of Transit Observation without Personal Error." By S. P. Langley.

Erratum. July 1, page 180, line 4, for *Jupiter* read *Saturn*.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To May, 1877.
Aldam, W.

To June, 1877.
Clarke, J. S.
Compton, A. J. S.
Knobel, E. B.
Lancaster, J. L.
Lancaster, W. L.
Ryle, Rev. Canon.

To July, 1877.
Grover, C.

To Oct., 1877.
Burton, C. E.

To Nov., 1877.
Hederstedt, H. B.

To Dec. 1877.
Guyon, G.

Metcalfe, Rev. W. R.
Remington, Major.

To March, 1878.
Hills, H.

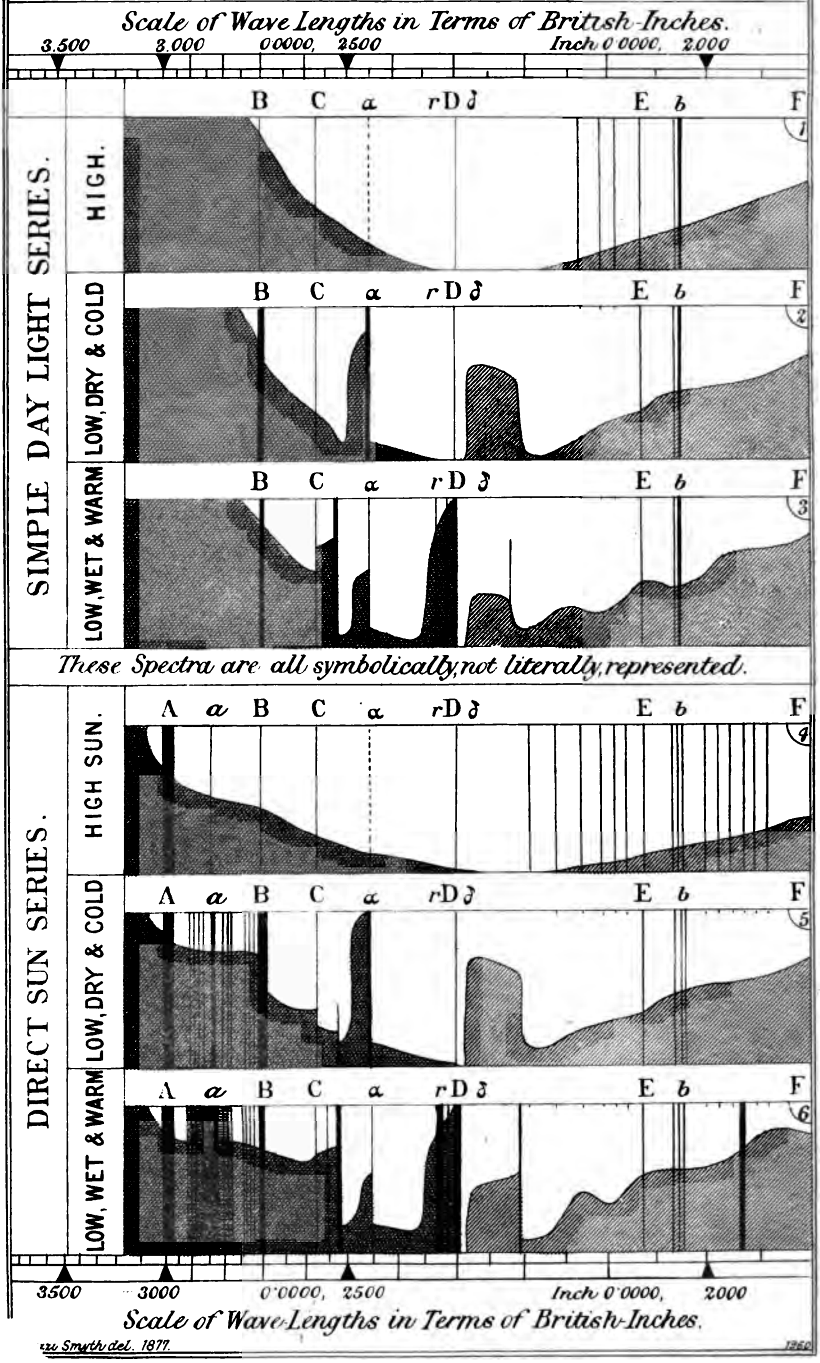
To June, 1878.
Howlett, Rev. F.
Tupman, Captain.

The Astronomical Register is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings** per Quarter, ***payable in advance***, by postage stamps or otherwise.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. Jackson, *Clarence Road, Clapton, E.*, **not later than the 15th of the Month.**

RED END OF THE SOLAR-SPECTRUM.

Part 1. Meteorological applications in the small and rough.



These Spectra are all symbolically, not literally, represented.

…zu Smyth del. 1877.

The Astronomical Register.

No. 177. SEPTEMBER. 1877.

THE SATELLITES OF MARS.

The following telegram has been received by M. Leverrier from Prof. Joseph Henry, Secretary of the Smithsonian Institute: "Two satellites of Mars, by Hall, at Washington, first elongation west, August eighteenth, eleven hours Washington, distance eighty seconds, period thirty hours, distance of second fifty seconds," which being translated from telegraphic English into ordinary language probably means that Prof. Asaph Hall, of Washington, making use of the great 26-inch refractor, believes that he has discovered two satellites to the planet Mars. The first was at its greatest west elongation on August 18th, at eleven hours Washington time. Its distance was then eighty seconds of arc from the centre of the disc, or in other words, when at greatest elongation it was only three diameters of the planet from the western limb. (The diameter of Mars at Greenwich noon on the 18th August was $23''\cdot75$.)

It would seem that Prof. Asaph Hall has observed this satellite sufficiently often to be able to predict that its period is only 30 hours. The distance of the second satellite is given as 50 seconds, but it does not appear whether it is really nearer or further from the planet than the other satellite.

OPTICAL SPECTROSCOPY OF THE RED END OF THE SOLAR SPECTRUM, AT A HIGH ALTITUDE IN THE SKY.

PART I.—METEOROLOGICAL APPLICATIONS IN THE SMALL AND ROUGH.

"All the most important features of the solar spectrum cluster about the blue and violet end thereof," wrote, not long since, a great spectroscopist, to whom we all owe much; and he may be right, for it is that end which chiefly represents the higher volatilizations of the intensest heat, all the most sensitive known reactions to actinic influences, with their train of permanent impressions of even the minutest visible and some invisible features, and the chief triumphs of human labours in both theory and observation.

But precisely because all that may be most absolutely true, and because the blue and violet spectrum end has, therefore, troops of able observers, devoting themselves magnificently to further researches therein; so a duty has fallen into my province, in my particular mode of trying to work, to the best public advantage, the small and poorly fitted-out Royal Observatory of Edinburgh, to take up the too generally neglected, even by some despised, red end of the solar spectrum, and endeavour to do a little more justice to its innate, natural claims to attention, in a manner also suitable, if possible, to the wants, and ready to serve the demands, of the present scientific day.

Hence, after arranging a spectroscopic apparatus peculiarly for work towards the extreme red rather than any other part of the spectrum, and having had this grand practical truth proved again and again, in spite of me unwilling, that for any really fine optical examinations of solar light it was pure waste of 9,999 out of 10,000 of precious moments of time spent upon it to attempt to make these observations in the latitude and locality of the Royal Observatory of Edinburgh, because that locality is condemned to a low sun, almost constant clouds, frequent rain, and completely eternal haze of dun-coloured, greasy, coal smoke, thrown out from legions of chimneys, both factory and domestic, close around the observatory's site, on Sundays, too, as well as week-days, and by night as well as by day—I therefore determined to try what could be done by taking the instrument for a time to some southern land.

Of course, to have got the best results, I ought, besides going south, to have followed Newton's sage, prospective advice of making the experiments in the pure and calm air on an elevated mountain top, raised far above the principal clouds and grosser impurities of the lower atmosphere; and much indeed had I desired in this cause to repeat the experiences of the Teneriffe Astronomical Mission of 1856. But as no public funds or other assistance of any kind appeared, and my private purse was already nearly run dry by the cost of the spectroscope itself (requiring, as it eventually did for travel, no less than four large boxes and three tripod stands), I could only make a compromise with opposing requirements; or rather found that I had only just enough money left, this year at least, to take the instrument by an economical sea-voyage to some southern city, where the climate should be proverbially dry rather than wet, where little stone coal is burned, and where the summer sun should rise nearly 20° higher in the sky than it ever does in Edinburgh.

Such a city proved to be Lisbon, and the means of getting there were the ships of the Liverpool Pacific Steam Navigation Company, forming quite a link of connection between the coasts of cloudy Great Britain and sunny Portugal—which many others besides an occasional astronomer and his wife might well profit by; though the two latter have also to acknowledge the handsome liberality of the Company, who conveyed all the spectroscopic packages both out in their s.s. "Aconcagua" and home in their s.s. "Cotopaxi" as luggage (in a case where no merchasdise is permitted), and without any charge. Such action on the part of the directors tended to make matters look cheerful, if not hopeful also, from the very moment of embarking. But even before that, we had been greeted with something connected with the red end of the daylight spectrum, which promised to be useful to others as well as interesting to ourselves, though it was but a hasty observation, and made in a very petty instrumental manner.

Small it was obliged to be, because neither in a railway carriage on shore nor in a vessel at sea can a large spectroscope of many feet long and broad be employed, any more than an astronomical telescope of the same size; and just as every navigator still depends in his nautical astronomy, on board the grandest ship that floats, upon a diminutive sextant which he holds daintily in his fingers, so the spectroscopist when travelling must content himself with the smallest conceivable spectroscope, in fact a "waistcoat-pocket spectroscope," after a fashion now made by several opticians.*

But what may any traveller hope to accomplish with such a little mite of a tool? Nothing that can touch the particular researches into details of the solar spectrum as they are carried on with large instruments in fixed observatories, you may be sure. But here is an example of what bountiful Nature produces from time to time out of her abundant store of wheel within wheel of many phenomena, if carefully and conscientiously observed.

---

* By no one better than Mr. Adam Hilger, 192, Tottenham Court Road, London. The particular example, whose use is presently to be described, was made by him; and was contained in a tube about 4 inches long and ½-inch in diameter only.

It was summer, and on the morning we left home, the 11th of June, a bright morning with a fairly high barometer (30·00) and a pleasant temperature of 64°, my wife had written down in her little meteorological journal, in the column entitled "Spectrum of daylight sky," and in its respective sub-columns of "Rain band" and "Low sun band," the figures 2 and 2, implying, where 10 was an expected and possible maximum, a very moderate degree of each of those effects in the red region of the spectrum. So we looked for a fine day, and a very fine day, too, it turned out, nor had either of us the least suspicion of anything else being literally in the wind, until, by the time the train was passing through the lake-district of Cumberland, we positively started on perceiving in the pocket spectroscope a rain band so strong as to be instantly marked down with a 5 to its account.

This was an observation made, be it remembered, when travelling at the rate of 20 miles an hour, and through a thick glass window; and being an uncomfortable feature in prospect, was attributed at first to *that* lake district where it is *always* raining, and where Mr. Isaac Fletcher, of Workington, has found, he believes, the rainiest mountain nook in all Europe. The lake district, however, was soon passed, but not the rain band, for that remained in the air and continued in the spectroscope, accompanying us into Liverpool; and though no rain fell there, and the clouds, barely hiding the sun's disc, were thin, yet the rain band at the red end of the spectrum was something that was a caution to behold, looking "like a black ribbon in the sky," said the lady, until darkness closed in.

And what came of it? Simply this, that next morning broke in steady unmitigated rain, pouring almost all the day, and making us thankful that the ship was not to sail until the day after.

That day, June 13, began dark and threatening, with heavy clouds. Half a regiment of soldiers passing before St. George's Hall carefully encased in their long grey great-coats showed that their commanding officer expected another rainy day. But we appealed to the spectroscope, and found that in spite of vulgar appearances there was no rainband among the clouds (the entry made in the journal was 1 only) and no rain fell. In fact the day turned out positively fine; the afternoon, as we went on board the s.s. "Aconcagua," was exceptionally fine, and an exquisite summer evening saw that grand 4,000 ton vessel gallantly passing the "Liverpool" outer light-ship and beginning her course to the south.

The application then of the spectrum rain-band rule, elaborated in Edinburgh, seemed thus far to suit the Liverpool locality also, but could it be extended much further?

We had not, indeed, proceeded very far southward in the "Aconcagua's" subsequent days of steaming, before we were compelled to remark the greatly increased strength of the rain-band; and though absence of city smoke partly explained that, and something was due also to moist sea air and rising temperature, yet not all. For through the whole of June 15th the rain-band rapidly increased in intensity until the lady entered it in her journal as 8, while towards sunset it looked so absolutely threatening (only in the spectroscope remember) that, in spite of clear sky, high barometer, and generally enjoyable circumstances, we thought it a duty to communicate with the captain as to coming rain; rain, too, in all probability of that irregular nondescript order which came down in force on the region of France drained by the Garonne two years ago, and washed away the southern half of the devoted city of Toulouse immediately after that immaculate mathematician, M. Le Verrier, had predicted fine weather for all the country.

The stars shone most brilliantly that night, and the new moon, with the old moon in her arms, was so extravagantly bright that it rather *burned* on the dancing wave tops than merely "weaved its chain of light," but next morning (June 16) when the captain's skilful navigation had meanwhile placed us in the mouth of the Garonne itself, a few miles below the thriving city of Bordeaux, that river, I believe always muddy, was pouring down liquid mud; and the sky—full of jostled masses of rent and heaped up steel-blue clouds, flying, torn into shreds, now here, now there under under divers currents—looked like a field of battle after the carnage is over. And so it was in a manner, for landsmen presently came off and told what a fearful night they had passed through with thunder, lightning and with rain; while as to the quality and weight of the latter, a second edition of it began soon after, and washed the ship's decks in most determined style.

Now remark, if you please, this second case of a distinct prediction of coming rain by the spectroscope, when the barometer said nothing, and the weather to most persons appeared simply delightful, and yet was hastening on to a perfect abyss of rainfall with thunder and lightning too.

How do you do it? asked a friend. Well, as you, or almost every one, might also do it, with proper attention to certain principles and a neat little spectroscope in your waistcoat pocket, ready at any moment to be pointed towards the light of any suspected part of the sky.

The first practical point to secure is, that the spectroscope's slit shall be clean, exceedingly, and very narrow, perhaps not more than the hundredth of an inch broad; though the real test is the visibility by its means of the fine dark lines, called by Fraunhofer, fixed lines; and by a grateful scientific world, "Fraunhofer lines," in the spectrum of the light of the upper sky, say at an altitude of not less than 30° or 40°. What is then to be seen is represented in our fig. 1, plate I., partly literally, as to the lines, and partly symbolically as to the strength of the background of light on which those vertical lines are exhibited—by the *depth* of the spectrum strip left unshaded. While similarly a portion of shaded space, rising up from the base-line of the spectrum, is intended to show a "band," or clouded nebulous streak, parallel to any of the Fraunhofer lines; and like them, in the reality, passing transversely up through the whole height of the spectrum strip; but shortened in height here, to denote in an easy manner by *height*, the *degree of intensity* of the band's shade; and made with sloping border, in proportion as such band in the spectroscope is shaded off laterally.

Hence from lines B to C (the red end of the spectrum) and C to D, the light of the spectrum-strip is continually increasing; and is still strong in the yellow and green from D to E; while from E to F, in the blue, it is fainter again. But at, and about, E and *b*, the Fraunhofer lines, originated in a manner in the sun, do exist spectroscopically, and you must alter the width of slit, or focus of eye-piece, until you see them at least as well as in our plate.

When you have got the instrument so adjusted within itself, fix it there; and in that state look with it at the lowest light of the sky that you can see in any azimuth; and you will have a spectrum presented to you, more or less like our 2 or 3. That is, while the solar lines E and *b*, if there is light enough to show them, are just like what they were before, a variety of new lines, or bands, or intensifications of old ones, have started into view chiefly at or near the spectrum's red end.

All these changes, sometimes of one kind, sometimes of another, are produced by the action of our earth's atmosphere on the daylight, primarily solar light, as it comes through the denser envelopes of air near the horizon. Some action of the same sort there may and must be with light from the higher part of the sky also; but it is in general barely, if at all, sensible; and the following little table, prepared by M. Marié Davy, of the Mont Souris Observatory, may be useful in reminding the observer of the immensely rapid increase of the atmosphere looked through, as he approaches the horizon.

Angle of observation.		Thickness of atmosphere looked through.
90° alt.	=	1·00
80		1·02
70		1·06
60		1·15
50		1·30
40		1·54
30		1·96
20		2·80
10	=	4·92
9		5·31
8		5·75
7		6·25
6		6·83
5		7·51
4		8·28
3		9·18
2		10·20
1		11·37
0	=	12·69

Any effect then which the atmosphere has to show will be more easily seen at the lowest possible observing altitude, if the air there be clear, and illumined by light *coming through it from beyond*. This is a very important discrimination to keep to,—for if the air, of such low strip of sky be, on the other hand, filled by particles of smoke or mist *reflecting*, in place of *transmitting*, light,—such reflected light, coming most probably from the higher part of the sky, will give the spectrum of that high part and not of the low portion, pure and simple, where it is observed at for the moment. Hence a clear transparent atmosphere, low altitude, and transmitted light, the stronger the better, are the ideals to be aimed at in meteorological spectroscoping; but must often be compromised and made up for in some way or another, when a particular observation *must* be made.

But the rain-band, where, or which is the rain-band, do you ask?

First look at fig. 2,—to accustom yourself to what is *not* the rain-band. That view shows the spectrum of several frosty evening skies observed last winter, when there was no appreciable moisture in the atmosphere, and when there could not, therefore, be any rain-band. The line B has thickened, and so has *a* (comparing fig. 2 against fig. 1), therefore, put them down as dry air, or telluric gaseous bands. A broad band has also appeared at *δ*, and is nearly of the same order, but with the addition of being so peculiarly dependent on the altitude of the sun (whether visible or not) that my wife styles it in her journal "low sun band," and under that name it says nothing either for wet or dry air, to whatever intensity it may be seen; and that greatest intensity is always, if the light remains strong enough, a few minutes *after* sunset, or when the sun's zenith distance is more than 90°.

Next, from fig. 2, turn to fig. 3:—and what is the most striking alteration?

Undoubtedly the appearance of lines and a band at *r*, on the left hand of D, and also on the right hand of C. These lines and bands are all produced by the vapour of water in the atmosphere; and as the group near D is the most conspicuous, *that* is the "rain-band," for meteorological purposes; and accordingly as it is seen more or less strong, other circumstances being the same, so is there more vapour of water, or material for rain in the atmosphere, and so *likewise* is its temperature probably higher; for the rain-band increases in strength regularly as we proceed from the colder to warmer parts of the earth.

Hence, to a certain extent, the optical observation of the rain-band is equivalent to a physical observation of wet and dry bulb thermometers, and to all the subsequent calculations from them of elastic force of aqueous vapour, grains of moisture in a cubic foot of air, &c., &c.

To illustrate this point let us compare the hygrometrical observations of the Scottish Meteorological Society in Edinburgh last winter, with the lady's spectroscopic meteorologic journal already alluded to.

January was latterly cold and dry; and the rain-band nowhere seen: or thus, numerically, during the last three days:—

Date.	Meteorological obs. and computations.				Spectroscopic notes	
	Dew point.	Elastic force of aqueous vapour.	Grains of moisture in cubic ft. of air.	Humidity, Satn. = 100.	Rain-band.	Low sun-band.
Jan. 29	33·3	·192	2·2	93·	0	4
„ 30	31·0	·174	2·0	80·	0	4
„ 31	34·3	·198	2·3	91·	0	2

The same was the case with February generally; though on one day therein, viz., 14th February, a strong rain-band was noted in the one journal; and the other journal, on being recently examined, had in its own way chronicled extra grains of moisture in the atmosphere on the same day; thus—

Date.	Meteorological obs. and computations.				Spectroscopic notes	
	Dew point.	Elastic force of aqueous vapour.	Grains of moisture in cubic ft. of air.	Humidity, Satn. = 100.	Rain-band.	Low sun-band.
Feb. 13	37·0	·220	2·6	86·	0	2
„ 14	44·4	·292	3·3	88·	3	2
„ 15	40·6	·293	2·9	79·	0	1
„ 16	35·9	·212	2·4	83·	1	3
„ 17	35·5	·207	2·4	84·	2	2

Is the spectroscopic rain-band then, a mere repeater of, and apology for not having, wet and dry bulb thermometer results?

By no means: for those thermometers are sensible to nothing but the state of a few inches of air immediately around them; while the spectroscopic observation gives an integration of all the miles and miles of atmosphere looked through; and it is in this manner, by dealing as easily and necessarily with the upper and distant, as the lower and nearer, strata of the atmosphere, when permeated by light passing through them in nearly horizontal direction to the observer,—that the spectroscope becomes capable of not only showing what is, but predicting what may be going to happen at the base of the atmosphere and the surface of the earth.

Sometimes, indeed, the two moisture instruments are apparently in conflict, but from this very cause. Thus, on Sunday the 15th of April there was all day long a stronger rain-band (= 5) than had been seen for many months, but no appreciable change in the wet-bulb thermometer was remarked; and nothing particular, except clouds driven by a *south* wind, was perceived in the weather of Edinburgh. Yet the papers recorded next morning, per telegraph, that rain had fallen heavily the previous night in the west of Scotland and in England; while still more notably the bulletin of the Paris Observatory exhibited in its map for the 15th that a most abnormal and grand cyclone was that day passing over the south of England, in a course from N.W. to S.E.; while on the 16th, 17th, and 18th there prevailed so heavy a storm in the

North Sea from the E.S.E., inaugurated in a manner by that cyclone, that the Firth of Forth was filled with vessels running in for protection.

But if such useful results are to be obtained from merely spectroscoping the air, clouds, and simple dull day-light, would not finer results be obtained—asks an enthusiastic friend—if you were to spectroscope the sun itself? To which I reply, Oh, by all means; whenever you can and do see the sun, spectroscope it too, by all means; and you may then get such results as are rudely attempted to be depicted in our Figs. 4, 5, and 6; but lay out your account at the same time for this,—the *high* sun will almost blind you, even through the narrowest slit, unless you use coloured glasses, and they introduce troubles of their own; while the *low* suns give you very little time for making any observations; for the intensest expression of what you wish to see in the spectrum there, is confined within the last minute only before the sun sets; and sets, too, not behind a hill or a cloud-bank, which will not do at all, but a genuine and a level, or still better, a depressed, horizon at *more* than 90° zenith distance.

If then you do effectively accomplish these solar observations you will be rewarded thus:—With a high sun there is a vastly increased number of Fraunhofer lines, sharp and precise to a degree; the spectrum is much extended in length, and in its new parts new lines appear, whereof the awfully colossal proportions of the one line called A, or colloquially "great A," is something for an intelligent man to have seen once before he dies.

With a low sun, again, let it be as scarlet in colour as you please, it falsifies a conclusion deduced from coloured stars by a great stellar spectroscopist; for it is precisely when the setting sun is most red that then the vigour and blackness of the spectral lines *in the red* is most striking; these lines being, however, without doubt, of telluric, or earth's atmosphere, rather than of solar-atmospheric, origin. But of all the lines which *may then* be seen there is none which, to me, at least, was more surprising than the change which had come over "*a*," colloquially "little *a*," in our Fig. 6, which gives a representation of what was observed in the setting sun off the coast of Spain on June 18.

In the high sun "little *a*" is little *a* indeed, as to its small visibility in any shape; but at set of sun what have we got here? At first I could not believe my eyes, and made quite sure that the huge black bar almost at the red end of the spectrum was "great A." But no; though I have not seen attention called to the circumstance elsewhere, great A was then a comparatively unimportant line in the further red and almost outer darkness; while "little *a*" had swelled up from the frog size to that of the bull, and had at last become positively elephantine in thickness and ponderosity; or it was even a case of a shrimp that had grown to be bigger than a whale. "Little *a*," moreover, having been assigned years since to "watery vapour" by the great Swedish spectroscopist Angstrom, behold, when it comes out in this manner, and in such weather as we had then on the North-west coast of Spain, why, behold, "little *a*" must also take its place among the spectrum's *rain-bands,* and as one of the most powerful of them, too.

Of what further followed that same evening in Vigo Bay; and how, wet as was the air under the lower dark clouds, and full of rain bands as was the spectrum of the western orange-red light of the sky beneath those clouds—yet, when presently, through an opening in them almost overhead, a brilliantly illuminated rose-pink red, upper and far more elevated cirrus cloud was seen, *that* gave but little rain-band; not much even of low sun-band, but a startling edition of *a*, a dry air band; and of such dry air, no doubt, as what we experienced 21 years ago on the Peak of Teneriffe as being the natural condition of the atmosphere in that latitude, at that season, *above the lower clouds.* How we marked that impressive appearance of the *a* band actually above the orthodox maximum, viz., at 12 instead of 10—of that, I repeat, and many other such things beside, did space allow, much more might be said, and to the interest, perhaps, of not a few readers; but that I am formally reminded on another side that we are bound to Lisbon; engaged moreover to deal there with something beyond mere waistcoat pocket spectroscopy, and to apply such larger apparatus to high angles in the sky only; for what has an astronomer to do with low altitudes and weather effects in the lower atmosphere?

They are no business of his, chime in the daily increasing legions of meteorological observers.

PIAZZI SMYTH.

Edinburgh: August 10, 1877.

ON A STELLAR SYSTEM WITH RAPID PROPER MOTION.

By Camille Flammarion.

The researches which I have undertaken on double stars and proper motions have led me to the discovery of various stellar systems. I designate by this name groups of neighbouring stars animated by a common proper motion in space; and it appears to me that they are destined to become of great importance in the knowledge of sidereal astronomy. These systems are certainly formed of stars physically associated; for the proper motion of the larger star is shared by the other, and in certain cases by several other companions, and often that proper motion is considerable. We meet with such systems not merely amongst double stars whose angular distance is only some seconds; they are found also amongst stars separated by several minutes, and even several degrees from each other. Astronomers are aware how difficult it is to disentangle the real value of a proper motion from the many sources of error which accompany the determination of the absolute positions of stars, and how cautious it is necessary to be before asserting the reality of a rapid motion; and still more so in the case of two or more stars which appear to be associated. It is on this account that I think it useful to indicate such groups, in order that special measurements, still more precise, may verify and confirm them.

The establishment of these measurements has a first-rate interest in the study of sidereal astronomy.

I will at once call attention to a system formed of two circumpolar stars that appear to be connected by a common rapid proper motion, although they are distant from each other more than six minutes of time in R.A., and more than ten minutes of arc in P.D., a fact all the more remarkable and interesting, as one of the two is double, and composed of two small components of no more than $1''\cdot 2$ of angular distance from each other. These stars have been the object of numerous observations. The first is the star 7510 B.A.C., of the the 5·5 magnitude. It is the same as 3511 Groombridge, 974 Pond, 22490 Oeltzen Argelander, 5299 Radcliffe*. Its position is the following, reduced to the equinox of 1880:—

R.A. 21h. 28m. 20s. P.D. $9^\circ\ 59'\ 55''$

The second is the star 2801 Σ, of the 7th magnitude. It is the same as 41971 Lalande, 3477 Groombridge, 5242 Radcliffe, but is not found in B.A.C. Its position, for 1880, is

R.A. 21h. 22m. 2s. P.D. $10^\circ\ 9'\ 50''$.

The difference of position, therefore, between these two stars is 6m. 18s. in R.A., and $9'\ 55''$ in P.D.

According to the reductions of observations the annual proper motion of the star 7510 B.A.C. is assumed as follows†:—

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* It might be identified satisfactorily with two stars of Lalande: 42215 and 42218 which seem to be only two observations of the same star. Their difference of positions is only 1s. in R.A. and $3''\cdot 7$ in P.D.

† See B. A. C. p. 51. Rad. Cat. 1860, p. 304, &c.

In R.A. +0·119s. In P.D. —0″·100.

That of 2801Σ has been estimated by W. Struve at Δ R.A. cos. D. = +26″·7 and Δ D. = +9″·3 for the secular motion.* Dividing the figure for R.A. by cos. D. and reducing into time, we find for the annual motion of the star, in R.A. +0·099s., in P.D. —0″·093.

On the other hand, meridional observations contained in the catalogues of Lalande, Groombridge, Σ Dorpat, and Σ Pulcowa,† give the following results:

	s.	″
Struve Lalande	+0·127	—0·160
Struve Groombridge	+0·126	—0·140
Pulcowa Dorpat	+0·078	—0·030

The mean of these four determinations is:

R.A. +0·108s. P.D. —0″·106

Thus the motions of these two stars are in the same direction, and almost of equal velocity, and much exceed the ordinary mean of proper motions. As we have just observed, the first of these two stars is single and the second is double. Their association, then, forms a *stellar system*, and this system presents a very great resemblance to the stars 30 Scorpionis and 36 A Ophiuchi, where a single star and a double star are similarly associated in a community of movements, although removed from each other by 12 minutes of arc.

It is worthy of attention that the direction of the motion we have been examining is nearly opposite to that of the translation of the Sun in space, and it is the same with that of the stars 30 Scorpionis and 36 A Ophiuchi. We may consider, therefore, that these motions are due in part to the perspective of our own displacement, and that perhaps these stars are not very remote from us.

The pair 2801Σ is formed of two stars (7·3 and 8·0) separated by only 1″·2 distance, and whose orbital movement is extremely slow.

It would be useful to verify by fresh observations the vastness and similitude of these two so remarkable proper motions.

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* Positiones mediæ, p. ccxxi.

† Positiones mediæ, p. 331, 347, and 363.

ANOMALIES OF VISION—AS AFFECTING THE OBSERVATION OF FAINT STARS, &c.

By William Noble.

I have heard it maintained as not improbable that in no two human beings does the same object produce a precisely similar visual impression. Now, although this is certainly a much too wide and sweeping assertion as it stands, yet there is a certain substratum of truth in it; and there can be no possible doubt that the deviations from normal powers of vision (whatever we may agree so to designate) are almost infinite. My more immediate object in the present paper, then, is to discuss some of these peculiarities in their relation to observations of faint or minute stars, and to point out certain ways in which such observations are affected by them.

And first as to colour-blindness, which is undoubtedly more common—in a very slight degree—than is at all popularly supposed. I am not speaking here of such cases as those of Dalton and others, by whom red and green, or blue and yellow, are wholly undistinguishable; but of persons whose perception of (let us say) the various shades of red, orange, or yellow is very slightly at fault. To such it is a standing puzzle how certain stars can ever have found their way into catalogues as "red" at all; for to them they are barely perceptibly of a yellowish tinge. On the other hand, however, there are undoubtedly observers, like Schjellerup and Schmidt, who seem to perceive a reddish hue in objects which fail totally to afford any such impression to the vast mass of observers generally. In the same way the colours on Jupiter's disc are perfectly distinguishable by some eyes, while others quite fail to perceive the bright orange, slate, and other tints which they are assured they ought to see distinctly. Not a few people, too, are unable to detect the ruddy hue of Mars, especially in the telescope; and such will often tell you, if you ask them what is the colour of Arcturus or of Aldebaran, that it is white. This, then, being an ascertained and admitted fact, it supplies one explanation of some of the curious discrepancies which we find in accounts given of the visibility of minute stars, by various observers. It may be regarded as certain that the *colour* of the minute comes to a large star has a very great deal to do with its visibility to any individual eye; and I think it will be found, as a rule, that in the majority of cases, blue stars are discernible of less magnitude than red ones—in other words that (*cæteris paribus*) the latter first cease to be visible.

I have just said that the varying susceptibility to the impression of colour by different eyes affords one explanation of certain discrepancies in the estimation of star magnitudes of the higher orders by different observers. It, however, by no means supplies the only one, as there undoubtedly does exist, in certain persons, an acuteness of vision which seems to have no assignable reference to colour (as such) whatever; but possibly to have its origin in an exceedingly accurate adjustment of the various parts of the eye, *inter se*, and in an abnormal sensitiveness of the optic nerve. The late Mr. Dawes supplied an instance of this; he, as is well-known, having been able to detect the companion to Polaris with an object-glass of only 1·6 inches in aperture. His delicate perception of planetary markings, &c., is also equally notorious. I mention this here, and may have again to refer to it further on, because a supposition exists that persons whose optical idiosyncrasy renders them peculiarly perceptive of faint planetary markings, are among those who fail most conspicuously to pick up very minute points of light. Coming, however, to a more recent example of extremely acute vision, I may remind your readers of the astonishment created among astronomers during the spring of last year by the announcement that Mr. Isaac W. Ward, an amateur observer residing in Belfast, had succeeded in seeing the two exterior satellites of Uranus with a Wray achromatic of only 4·3 inches in aperture![*] I must confess at once, that I for one received this announcement originally with great incredulity; persuading myself that Mr. Ward had either been the victim of an optical illusion, or that he had picked up some exceedingly small stars in the neighbourhood of the planet. Upon consideration though of the evidence adduced, and from other information which I acquired regarding Mr. Ward's most wonderful power of detecting objects of the last degree of faintness (but concerning whose existence there could be no doubt whatever), I ultimately arrived at the conclusion that he actually must have seen the excessively faint objects which he claimed to have perceived. An even more extraordinary case than that of Mr. Ward has, however, been brought to my notice by an esteemed correspondent of mine, the Baron von Ertbon, of the Chateau de Solhof, Aertselaer, near Antwerp; one of an energetic band of Belgian amateur astronomers in that neighbourhood. It is that of his own nephew, a lad of 13, whose powers of vision appear to approach more nearly to the miraculous than those of any one of whom I have ever hitherto heard. Premising that his astonishingly acute sight had from his earliest infancy attracted the wonder of his family generally;

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[*] R. A. S. *Monthly Notices*, Vol. XXXVI., p. 294. *Astronomical Register* Vol. XIV., p. 106.

attention to it, in connection with the perception of celestial objects, would seem to have been first directed in November, 1875. On a certain evening in that month, while walking with his uncle, he, pointing to the Pleiades, exclaimed, "I see seven large and seven small stars there." Extraordinary as this appeared to the Baron von Ertbon, he did not follow it up until the August of last year, when discussing the matter with M.M. Houzeau and de Boë, the latter suggested that the boy should be asked to look at the Pole Star (this time of course with the telescope) since he, M. de B., believed that he had discovered two new companions to that star (with a 6·3 inch object-glass) but had not yet dared to make his doubtful discovery public. The juvenile incipient astronomer—who had never even heard the pole star spoken of!—was therefore requested to examine it, with a 4·25-inch Steinheil objective. At once on putting his eye to the telescope he picked up the larger of the two new comites (that at an angle of about 300°) and four nights later detected the fainter one with almost equal ease. He has since seen both these objects repeatedly, and even when the field has been slightly illuminated. It may not be unworthy of remark that his vision of these particular objects is better with a power of 100 than with higher ones. It would be foreign to my purpose here to detail the various precautions employed and instrumental changes made to secure him from being the victim of any illusion: suffice it to say that they were both numerous and efficient. He sees the new companions to Polaris better in the twilight than when it is dark, but in this he is by no means singular. It is unnecessary to add that several English observers found these comites with adequate instrumental power when M. de Boë ultimately published his discovery. With his left eye (which seems very slightly superior to the right) this gifted young Belgian sees β Equulei quadruple, 70 Ophiuchi with two minute companions, the companion to μ^2 Herculis, 178 Delphini quadruple, &c., &c., and on Sept. 6, 1876, saw easily and distinctly numerous bands on Saturn, the dark ring, the main or Ball's division, and another *interior* to it on ring B, which was more apparent in the eastern ansa. (The major and minor axes of the outer rings, be it observed, being at that epoch, $42''\cdot77$ and $6''\cdot07$ respectively.) A remarkable thing in connection with his sight is that with the naked eye he sees stars shorn of all the rays which surround them, as viewed by ordinary observers. Moreover, large flocks of larks pass over Antwerp, and he sees and *counts* them long before the country people with their ordinarily good sight can even perceive them at all.

It is almost needless to point out the bearing of such facts as these upon the subject of stellar photometry. Grave inconvenience and confusion have arisen (especially as regards stars of the higher orders of magnitude) from the varying light ratios adopted by different observers; but such inconvenience and confusion must be increased indefinitely, if we further introduce the element of uncertainty into the determination of our initial standard magnitude. Carrington, for example, calls a star just certainly visible with one inch of aperture a 9th magnitude. Pogson, on the other hand, speaks of the minimum visible with the same aperture as a 9·2 mag. Let us imagine, however, that Mr. Ward or the Baron von Ertbon's nephew (or in fact anyone whose sight approximates to theirs in keenness) were set to make this determination, I wonder what sort of 9th magnitudes we should arrive at then? And this suggests the idea that in the present confused state of stellar photometry it would be wise—at all events temporarily—to decide upon some conventional standard as our initial point. A recognised individual star would answer this purpose better than anything, were we only sure of its absolute invariability. This difficulty, however, would decrease in a high ratio were several stars of identical magnitude to be selected, and there would be no insuperable obstacle to this. At present compilers of catalogues and atlases are by no means even all agreed as to what constitutes a star of the 6th magnitude!

I throw out the few remarks contained in this essay merely tentatively, and to invite suggestions and discussion; and I have confined them almost entirely to a single effect arising out of the curious diversity which obtains in the visual powers of the human eye. I have purposely avoided all reference to what is best known among astronomers as "personal equation," I mean the difference of perception of the *time* of the occurrence of a phenomenon; because I believe that this has its origin by no means in the eye itself, but in the grey matter of the brain.

THE ATMOSPHERE OF THE PLANET JUPITER.

By E. Neison.

In the report of the Council to the fifty-seventh annual meeting of the Royal Astronomical Society, an extract is given from a then unpublished paper by Mr. Todd, of the Adelaide Observatory, and containing an account of some peculiar phenomena attending the occultation of the satellites of Jupiter. This extract was to the following effect:

"A very interesting phenomenon observed more than once, independently by Mr. Todd and by his assistant Mr. Ringwood, occurred when a satellite was on the point of occultation. Instead of disappearing gradually behind the planet, it was apparently projected on the disc, as if viewed *through* the edge of the planet, supposing the latter were surrounded by a transparent atmosphere laden with clouds. This curious phenomenon was noticed on two occasions at the disappearance of the first satellite, when it was thus distinctly visible on, or through the edge of, the disc for about two minutes before it was finally occulted" (*Monthly Notices*, Royal Astronomical Society, Vol. xxxvii., p. 198).

On the basis of this extract from Mr. Todd's paper, an eminent astronomer wrote an interesting article in the *Spectator*, in which he claimed these observations as a direct positive proof, from observation, of the correctness of the view of the constitution of the planet Jupiter, which had been advanced some years previously by Mr. Proctor.

In this article it was held to be established beyond doubt, that the phenomenon observed by Mr. Todd and his assistant, Mr. Ringwood, showed that the satellites of Jupiter could be seen through the atmosphere of the planet. Further, it was claimed that this demonstrated in a most positive manner that the atmosphere of Jupiter must extend to a depth of many thousand miles beneath the visible cloud bearing strata. Shortly after the appearance of the article in the *Spectator*, the same view of the meaning to be attached to Mr. Todd's observations, was advanced in the *Popular Science Review*, for April 1st, 1877, under the head of Summary of Astronomy. In this article, likewise, these observations were claimed by the same eminent astronomer* as the experimental demonstration of the view of the constitution of the planet Jupiter advocated by Mr. R. A. Proctor.

As up to this period Mr. Todd's paper had not been published, so that the basis of these decided opinions could, it is presumed, only have been the short summary already quoted, it was hazardous to have founded such extensive claims on so imperfect data. Nevertheless, these views deserve to have met with more general attention than was really given to them. For if these observations really prove the correctness of the views advocated, and do show without a question that Jupiter possesses an atmosphere of from five to fifteen thousand miles in thickness, a most important advance in science would have been effected.

Subsequently, in May, 1877, was published the April number of the *Monthly Notices* of the Royal Astronomical Society, and this contained Mr. Todd's paper, entitled, "Observations of the phenomena of Jupiter's satellites, at the Observatory, Adelaide, and Notes on the physical appearance of the planet." By Charles Todd, Esq., C.M.G. In this paper Mr. Todd described the phenomenon in question in the following words:—

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* Although in conformity with usage neither of these two articles was signed, they are anonymous merely in form, as the authorship is distinctly claimed, when they are stated to be by the author of papers acknowledged to be from the pen of a well-known and deservedly eminent astronomer.

"On one or two occasions when a satellite has been on the point of occultation, it has appeared to be projected on the disc of the planet, or rather, as it appeared independently to my assistant (Mr. Ringwood) and myself, as if seen *through* the edge of the planet, as if the latter were surrounded by a transparent atmosphere laden with clouds. I noticed this on two occasions, viz., on August 21, at the disappearance of the 1st satellite, when the satellite was distinctly seen, on or through the edge of the disc for about two minutes before disappearance—I say *through* the edge of the disc because the satellite was less bright than on a similar position at a transit—and again at the disappearance of the 2nd satellite on August 28. Mr. Ringwood also noticed it on August 12 and August 19, at occultation of 1st and 3rd satellite. In each instance the planet was splendidly defined, better than I ever remember to have seen it in England. It was not noticed at any reappearance which took place at the eastern or defective limb; the gibbosity of the planet was, however, quite sensible at reappearance when the planet was near quadrature—*vide* Oc. R. of 3rd satellite, on Aug. 19, and of 2nd satellite on Aug. 21, when the satellite at the instant of emergence was sensibly separated from the illuminated edge of the planet." (*Monthly Notices*, R. A. S., vol. xxxvii., p. 285.)

In a subsequent portion of his paper Mr. Todd gives additional details about these observations, and from this portion the following extracts have been made:

Aug. 12. Occultation of I. satellite. Disappearance.
Observer Ringwood.

		h.	m.	s.	
88.	External contact	0	45	15·42	G.M.T.
89.	Bisection (?)	0	46	47·42	„
90.	Disappearance	0	49	37·42	„

And to this last observation is appended the remark—"90. For several seconds before disappearance the satellite seemed to be visible inside the limb of planet, as if projected on planet."

Aug. 19. Occultation of III. satellite. Disappearance.
Observer, Ringwood.

		h.	m.	s.	
115.	External contact	21	53	55·00	G.M.T.
116.	Bisection (?)	21	58	19·00	„
117.	Disappearance	22	5	25·00	„

And to these observations are appended the notes:—“ 115. Very steady and well defined. 116. Appeared as a well defined nipple, protruding from planet; very steady and well defined. 117. Very good; steady and well defined; suddenly disappearing; satellite appeared as if projected on planet, and in internal contact a full minute before its final disappearance.”

Aug. 21. Occultation of I. satellite. Disappearance.
Observer Todd.

		h.	m.	s.	
124.	External contact	21	9	30·94	G.M.T.
125.	Bisection (?)	21	11	20·24	„
126.	Disappearance	21	13	36·24	„

And to these three observations bracketed together is added the remark, “Very good; steady and well defined; after bisection the satellite was seen as if projected on limb of planet.” This is the observation which in the previous portion of the paper Mr. Todd states the satellite was seen projected on the disc of Jupiter for about two minutes.

Aug. 28. Occultation of II. satellite. Disappearance.
Observer Todd.

		h.	m.	s.	
141.	External contact	22	6	34·30	G.M.T.
142.	Bisection (?)	22	8	8·00	„
143.		22	9	44·30	„
144.	Disappearance	22	11	38·59	„

And to these observations are appended the remarks, “ 148. Satellite quite distinctly seen, as if projected on planet. 144. Last seen.”

In all these observations a power of 200 was used on the fine 8-inch equatorial of the Adelaide Observatory. It will be remarked that the line at which the satellite is bisected is so difficult to ascertain accurately, that it is marked doubtful by a note of interrogation. Further, it was only when the observing conditions were good that the phenomenon could be seen, it having been detected in only four out of nine disappearances.

The appearance detected by Mr. Todd is one which has probably been noticed by most observers, who have been engaged in watching the phenomena of Jupiter's satellites with adequate telescopic means. It does not appear, however, to have been seen with the distinctness and certainty with which it was observed by Mr. Todd and Mr. Ringwood. On two or three occasions an apparent projection of the satellite on the planet's disc has been suspected with an aperture of only 6 inches. This appearance has been, however, regarded as merely deceptive. It must not be forgotten also that the same appearance has frequently been noticed in the case of occultations of stars by the moon. In this last case, though, it does not seem to have been ascribed either to the moon being surrounded by a dense highly illuminated cloud-bearing atmosphere, or yet to its being seen through the limb itself, such as might be supposed possible were the moon to possess a transparent limb—a crystal atmosphere in fact. The entire accuracy of Mr. Todd's observations must be admitted, it only remains to decide on the interpretation to be put on them.*

The question to be considered, therefore, may be summed up as follows: Given the phenomenon described by Mr. Todd, does it indicate the existence of a deep atmosphere to the planet Jupiter, or is it to be explained in some other manner? The most important point is evidently the answer to be given to the first portion—does or does not the apparent projection of the satellites of Jupiter on the disc of the planet prove that Jupiter possesses an atmospheric envelope of great depth?

In considering questions connected with the atmosphere of a planet, it must always be remembered that the theory of gravitation, and the known laws governing the behaviour of gaseous bodies enables the true constitution of any atmospheric envelope to be calculated with the greatest care and certainty within very approximate limits. It is not as if we could imagine some fanciful atmospheric conditions, and then boldly declare that this state of things existed on this or that planet. There is no difficulty in applying crucial tests, to any such hypothesis and if it does not fulfil certain conditions prescribed by known physical laws, it can be declared with absolute certainty that whatever may be the state of the atmosphere of the planet, that particular hypothesis does not represent it. In fact, there is no difficulty in ascertaining with very considerable accuracy the exact condition of the atmospheric envelope of any of the planets. The mathematical solution can be found, all that remains to be done is to ascribe to certain fixed constants their numerical values. It is true that these numerical values are not accurately known, but we do know that they cannot exceed certain given limits.†

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* The only prior published observation of the phenomenon seen by Mr. Todd appears to be that of Mr. Hodgson, the late well-known astronomer, who on January 3rd, 1857, saw the I. satellite projected on the disc of Jupiter and with a clear space around it for nearly a minute. This observation was made with his 6 inch equatorial, and corresponds exactly to what was seen by Mr. Todd.

† It is exactly as if we wished to know the height of a person living in London, so as to build him a house. It would be absurd to suppose that his height might be less than one foot or more than twenty feet high (to take rather wide limits). To suppose him either would be to infringe conditions which we know cannot be infringed, namely, that the fact of his existence would not have remained unknown.

If, then, it can be shown that to suppose that Jupiter possesses an atmosphere of so great a depth as 2,000 miles, is to assume a condition of things on the planet which is entirely inconsistent with known physical laws, then this must be held to invalidate any hypothesis which would make the depth of atmosphere 5,000 miles, and still more so any supposition that the atmosphere was 10, 15, or 20,000 miles in depth.

In the *Quarterly Journal of Science* for October, 1875, it has been shown that, supposing the atmosphere of the planet Jupiter to be of the same temperature as our own, it is impossible it can have a depth of 50 miles, but that if we suppose the surface to be of an intense white heat, then the depth of the atmosphere may be as much as 80 miles. The depths are supposed to be taken below the extreme upper cloud-bearing strata of the atmosphere, and the atmosphere to have a standard density similar to our own, that is to say, it possesses a density similar to what would be the density of a like volume of atmospheric air under the same conditions. Now, by supposing various extreme conditions to hold on the planet Jupiter, the greatest possible depth can be found. First, suppose an intensely hot surface, far greater than the most intense heat ever artificially obtained on the earth. By this means the greatest depth of atmosphere possible might be increased to 200 miles. Secondly, suppose also that instead of being composed of constituents similar to the atmosphere of the earth, that it is composed of some entirely unknown substance of much less weight. If both these conditions simultaneously held, then the atmosphere might have a depth of 700 to 800 miles, or even by adopting the most extravagant supposition, be as much as 1000 miles deep. Still, it should be remarked that it is inconsistent with known facts, that these two conditions should be assumed to be co-existent, for really they both could not exist together. Both are likewise inconsistent with the telescopic appearance of the planet, which certainly neither rivals the sun in brilliancy, which its supposed immense temperature would make it, nor is free from clouds in its atmosphere which the other assumption would make it. At all events, however, we arrive provisionally at the conclusion that the greatest depth which the atmosphere of Jupiter can possess is not greater than 1,000 miles.

(*To be continued.*)

REVIEW.

Mr. George Calver has just issued a new and entirely revised and enlarged edition (with prices and a well illustrated catalogue) of his *Hints on Silvered-Glass Reflecting Telescopes;* a careful study of which we commend to those who may be desirous of purchasing a good, useful, and powerful reflecting telescope, at a very moderate price.

The author of "*Hints*" is evidently determined to keep pace with the requirements of the age, for we notice that he is prepared to construct reflecting telescopes, both on the "Newtonian" and "Cassegrain" principle, holding mirrors of no less than 36 inches in diameter, with driving clocks and everything complete. Now, when we consider that in order to obtain the *best results* on specula (large or small) the skilled optician must work "away from the tremor of a town," and that even the temperature of the laboratory and the polisher must be the same, we do say that great thanks are due from the observer to the maker of a really first-class reflecting telescope.

Mr. Calver has, perhaps, been a little tedious in his description of the processes of grinding and polishing the mirror, and obtaining a correct "flat;" but he has, doubtless, taken this trouble in order to help, if possible, the plucky attempts of clever amateur opticians in their very arduous endeavours to construct a reflecting telescope with their own hands.

We know that Mr. Calver has produced many very fine specula, both large and small, and we think that his new book shows an honest determination on his part to obtain still further improvements, both on the telescopes themselves and their mountings.

It is a fact that of late years "reflectors" have rapidly gained favour in the estimation of the astronomical public, partly, no doubt, from their beautiful definition of the planets and their easy resolvability of minute stars; and these fine results are owing to the skill and perseverance of such opticians as With, Browning and Calver; we, therefore, wish all of them as much success as they deserve.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

THE ROYAL ASTRONOMICAL SOCIETY.

Sir,—Through the kindness of a friend, I have received here a copy of the *Astronomical Register* for August, which contains a series of comments by Captain Noble and Mr. Proctor on some remarks of mine inserted in the previous number of the *Register*. I had intended to decline making any further communication on this deplorable subject, wishing rather to let the question remain in the hands of the Fellows, to be decided by them in such a manner as shall prevent in future any cause for a continuation of those personal animosities which are so evident to all after perusing Capt. Noble's letter. But I cannot refrain from offering a remark or two on certain matters alluded to in the letters of the above gentlemen, which I do in all sincerity, and with no other purpose than to lay before the Fellows the true facts of the case.

1. With regard to the Royal Astronomical Society's Club. I have now been a member of that Club since 1868, and I do most sincerely say that the conversations carried on at the meetings are of the most harmless character, and I cannot remember being a party to any "plot" against either Captain Noble or any other member of the Council. Surely a company of scientific gentlemen may be allowed to associate together without "hatching" plots against their colleagues. I am certain our time is spent in a manner very similar to that at another club to which I have had the privilege of being invited on several occasions, and at which I have invariably met my critical friend Captain Noble, who appeared to enjoy his dinner as well as any one else. These charges which he is persistently making are positive nonsense, and I hope that in future he will see the error of his ways and acknowledge it.

2. I am rejoiced to find that both Captain Noble and Mr. Proctor have reduced the "Club-party" by excepting the names of Sir George Airy, Professor Adams, and others. Perhaps they may be surprised to hear that both these gentlemen, whom they now so delight to honour, were strong supporters of the proposed amended Bye-laws, and to the former the Council were indebted for a series of most valuable suggestions, many of which were adopted by the committee appointed to consider the subject. Further than this, I am sure Sir George Airy will not object to my saying that he was one of the originators of the proposed alterations, and at his request I consented to bring the subject before the Council. I know that I am also correct in saying that Professor Adams deeply regretted the postponement of the question at the last special meeting, without allowing the promoters to explain the reasons why the amendment of the Bye-laws was recommended. Indeed he expressed to me his strong opinion that a direct vote of want of confidence had been passed, and that it became a matter of consideration whether the promoters of the meeting ought to resign their positions on the Council.

3. I trust Mr. Proctor will not consider that my reference to an anonymous writer reflected unfavourably on gentlemen who are in the habit of communicating their thoughts and experiences to the world anonymously; far from it, for no one is convinced more than I am of the necessity of that class of writing, when under proper control. But I *do* most strongly object to anonymous writers imputing unworthy motives affecting the character of private individuals, when these motives only have their origin in the perverted minds of the writers, who are evidently always on the look out for some imaginary indiscretion on which they may exercise their ill-nature. The remarks of such men should be treated with that scorn and contempt which they deserve.

4. It is far more interesting to me to spend my vacation holiday among the hills and dales of west Cornwall than to write a controversial letter, but I think I ought not to pass over some of the misrepresentations contained both in Captain Noble's and Mr. Proctor's letters, without giving at once one or two facts which I hope will modify many of the conclusions to which they have arrived, and which they are desirous should be adopted by the general body of the Fellows.

5. So far as I am concerned I hope this will be my last communication to you on the subject, and I am fully prepared to bow to the decision of the Fellows in February next, whether it be in favour of the management

of the Society by the majority in the present Council, or by a Council composed entirely of fresh blood. My opinion, however, is that an astronomical society ought to be governed partly by working astronomers, and that it will be a misfortune for the Society if they form no part of the future Council.

Truro: 1877, August 6. Yours truly,
EDWIN DUNKIN.

Sir,—As you invite communications from the Fellows of the R. A. S. respecting the somewhat extraordinary proceedings at the Special Meeting of the Society last June, I offer you a few remarks from one who has been a Fellow (with Mr. Proctor I reject the expression "outside Fellow" as a most unwise one) for the last fifteen years, and a constant attendant at the meetings for the whole of that time. I went to the Special Meeting in question prepared to give a general support to the proposed alterations in the Bye-laws, subject to such modifications as should seem desirable after discussion by the Fellows present. For, as I subsequently took an opportunity of saying, it appeared to me that alterations in the laws and regulations of a Society, although they might be from time to time expedient, should always be looked upon as a serious matter, and not "entertained or taken in hand lightly," but adopted after full and free discussion by all its members. When, then, it was pointed out by Lord Lindsay that such discussion was not open to us on that occasion, by reason of the want of due notice, I at once felt that I for one could not vote for making the said alterations then and there. What was my astonishment when a Vice President and late Secretary of the Society rose, and, with a manner of righteous indignation, almost suggesting that he had recently been out witnessing some Turkish atrocities in the East, told us that such were the propositions of the Council, and the Fellows generally ought to be "ashamed of themselves" if they did not endorse them at once as they were. Had every member of the Council been of *this* opinion (and we had good evidence that such was far from being the case) I for one could not have accepted it. I apprehend that no government in this country would last a week if it expected its administrative measures to be passed at once by the House of Commons without power to make any modification in them, nor would any Society or legally constituted body of gentlemen, associated for any purpose, accept any such claim on the part of its committee of management. I also failed to see how the suggestion of another member of Council on that occasion, that we might "talk about" any modification of the proposed alterations, without producing thereby any practical effect, met the case. I voted, therefore, in accordance with my ideas without any intention of expressing want of *general* confidence in the Council, merely wishing that the proposed alterations should be postponed till we were able effectively to consider them. My belief is that a large majority of the Fellows present were of the same opinion as myself.

I remain, Sir, yours faithfully,
Blackheath: August 6, 1877. W. T. LYNN.

D'ARREST'S COMET.

Sir,—The following is a continuation for the month of September of the ephemeris, by M. Leveau, which I sent you last month.

	R. A.		Decl.
	h. m. s.		° ′ ″
September 1	4 33 23	...	+3 58 48
3	4 35 34	...	3 44 6
5	4 37 37	...	3 28 58
7	4 39 31	...	3 13 25
9	4 41 17	...	2 57 30
11	4 42 55	...	2 41 12
13	4 44 24	...	2 24 35
15	4 45 45	...	2 7 40
17	4 46 57	...	1 50 29
19	4 48 0	...	1 33 4
21	4 48 54	...	1 15 26
23	4 49 40	...	0 57 38
25	4 50 16	...	0 39 42
27	4 50 44	...	0 21 40
29	4 51 2	...	+0 3 34

The comet continues slowly to approach the earth. Its distance therefrom on the day of its discovery was about 145,900,000 miles. Its distance on Sept. 29 will be 130,200,000 miles.

Northfield Observatory,
Eastbourne: August 1, 1877.

Your obedient servant,
G. F. CHAMBERS.

MINOR PLANET VESTA.

The minor planet Vesta, the fourth of the series, will come into opposition on the 15th of the month.

	Right Ascension.	Declination.
	h. m. s.	° ′ ″
Sept. 1	0 3 34·56	S.11 41 35·8
„ 7	23 58 40·33	S.12 28 54·7
„ 14	23 52 25·92	S·13 20 27·4

DISCOVERY OF A NEW PLANET (173).

A new minor planet (173) was discovered on the 1st August, at 10 o'clock, by M. Borelly, at Marseilles. The first exact observation of it was made on the 2nd. It is of the 10th mag. The minor planets (171) and (172), also discovered this year by M. Borelly, have been named Ophelia and Baucis.

Letter of M. Stephan, Director of the Marseilles Observatory, to the Editor of the *Astronomische Nachrichten.*

I have the honour to forward you herewith our first two complete observations of the planet (173), recently discovered by M. Borelly.

The planet was originally seen at 10 p.m. of the 1st instant. It was not, however, possible to continue the observation that night. The earliest accurate position refers to the 2nd of the month.

The planet is of the 10th magnitude.

Marseilles: August 4, 1877. (Signed) E. STEPHAN.

1877.	Marseilles Mean Time.	R.A. app.	P. app.
Aug. 2	12 44 54	22 40 25·45	97 35 27·6
3	12 47 33	22 40 5·48	97 45 29·5

(Translated from *Astronomische Nachrichten*, No. 2145.)

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN SEPTEMBER, 1877.

By W. R. Birt, F.R.A.S., F.M.S.

Last month we gave a few observations of an object which since the year 1860 has presented at different times aspects that induced the belief in some sort of variation in its appearance, dependent not so much on physical change in itself as on a something capable of altering its appearance as seen through the telescope; the appearances which have been successively seen are as follows:—a somewhat large cloud-like white patch on the dark ground of the Mare Crisium, west of Picard, in which nothing could be detected—a depression of a pit-like character of a peculiar whiteness in or *over* which nothing of a misty or cloudy character could be seen—the discovery in December, 1864, of two minute craterlets in this pit-like depression—variations in size, brilliancy and outline of the bright spots when seen, and occasionally a disappearance of the whole spot altogether—also the observation of one or both craterlets at irregular intervals. These appearances have more or less impressed themselves upon our mind during an interval of eighteen years, this present writing is from memory, it is simply reminiscences of the spot during the last eighteen years written without referring to the numerous notes which we have of it, they mark it out for *constant* observation. Its symbol is I C μ 16. Although the last cry of science is "The moon is dead," we have in this spot a good starting point for continued research into the physical condition of the moon's surface, it holds the place in physical selenography that the first determination of the orbit of a double star held in the fascinating study of binary systems.

The latest observation which has reached us of this curious and interesting object is dated June 15, 1877. The prominent feature observed is so very different from all that has hitherto been seen, that we hope increased attention will be bestowed upon it, especially by owners of large instruments. We give the record of the observation *verbatim* as written at the telescope by the observer, Capt. Noble.—"Friday June 15, 8h. to 8h. 15m. L.M.T. I examined the Mare Crisium with a power of 154, and attempted a rough sketch of its eastern boundary XXX. My chief object in making this note is to record the fact that at moments of the best vision the white spot W. of Picard was seen as a *ring* as though it were an excessively shallow large crater. Moon's age, 4·24 days." Capt. Noble's instrument is about 4½-inch aperture. On the same evening we recorded the appearance of the spot as seen with the R. A. S. Sheepshanks' telescope No. 5, aperture 2¾-inch, "Faint but discernible." Capt. Noble says, "So far from my notes made with the object actually under my view. I may add here that the spot presented something of the appearance which I have observed in Linné in 1869, Jan. 19, and on other occasions." We shall await with much interest the arrival of more recent observations.

A correspondent of the *English Mechanic,* T. Godfrey, calls attention to two objects on the moon's surface, viz., *Petavius* and *Thebit*; very little attention has hitherto been given to these objects in the way of observing the minute detail. Does the cleft on the east *cut through* the east rampart?

Buckhurst Hill: August 18, 1877.

Book received.—Report of the Savilian Professor of Astronomy to the Board of Visitors of the University, Oxford, June 19, 1877.

ASTRONOMICAL OCCURRENCES FOR SEPTEMBER, 1877.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Mars. —
Sat	1	10 0 10 15 16 38 17 26	Occultation of B.A.C. 1746 (6½) Reappearance of ditto Occultation of 136 Tauri (5) Reappearance of ditto			12 27·9
Sun	2		Sidereal Time at Mean Noon, 10h. 46m. 57·46s.			12 22·9
Mon	3		Sun's Meridian Passage 0m. 51·70s. before Mean Noon	2nd Tr. I. 2nd Sh. I. 2nd Tr. E.	7 36 10 11 10 24	12 17·9
Tues	4					12 12·9
Wed	5	12	Opposition of Mars to Sun	2nd Ec. R. 1st Oc. D.	7 14 36 9 29	12 7·9
Thur	6			4th Sh. I. 1st Sh. I. 1st Tr. E	7 40 7 55 8 55	12 2·9
Fri	7	1 0	○ Full Moon Eclipse of the Sun invisible at Greenwich Saturn's Ring: Major axis=43″·40 Minor axis=1″·73	1st Ec. R.	7 29 7	11 57·9
Sat	8	20	Conjunction of Moon and Mercury 0° 9′ S.			11 52·9
Sun	9	2 13	Opposition of Saturn to Sun Conjunction of Moon and Venus 4° 37′ N.			11 47·9
Mon	10			2nd Tr. I.	10 8	11 42·9
Tues	11					11 37·9
Wed	12			2nd Ec. R.	9 52 18	11 32·9
Thur	13	23 7	☽ Moon's First Quarter	1st Tr. I. 1st Sh. I.	8 33 9 51	11 28·0
Fri	14	5	Conjunction of Moon and Jupiter 4° 55′ N.	1st Ec. R.	9 24	11 23·1
Sat	15		Illuminated portion of disc of Venus=0·812 Illuminated portion of disc of Mars=0·995 Sidereal Time at Mean Noon 11h. 38m. 12·66s.	1st Sh. E.	6 37	11 18·2

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Mars.
Sun	16		Sun's Meridian Passage 5m. 18·50s. before Mean Noon			11 13·3
Mon	17	7 33 8 44 8	Occultation of B.A.C. 7049 (6) Reappearance of ditto Jupiter at quadrature with Sun	3rd Ec. R.	8 43 3	11 8·5
Tues	18	8 34 9 25	Occultation of 30 Capricorni (6) Reappearance of ditto			11 3·7
Wed	19			2nd Oc. D.	7 3	10 58·9
Thur	20	17	Conjunction of Moon and Mars 6° 48′ S.			10 54·1
Fri	21	12 26 13 32 3	Occultation of 14 Piscium (6) Reappearance of ditto Conjunction of Moon and Saturn, 4° 2′ S.	2nd Sh. E. 1st Oc. D.	7 29 7 46	10 49·4
Sat	22	3 34	○ Full Moon	1st Sh. I. 1st Tr. E. 1st Sh. I.	6 15 7 14 8 32	10 44·8
Sun	23					10 40·2
Mon	24	7 36	Near approach of 104 Piscium (6½)	3rd Oc. R.	7 29	10 35·6
Tues	25					10 31·0
Wed	26	11	Inferior conjunction of Mercury and Sun			10 26·5
Thur	27		Saturn's Ring: Major axis=43″·16 Minor axis=2″·31			10 22·1
Fri	28	16 49 17 58	Occultation of B.A.C. 1746 (6½) Reappearance of ditto	2nd Sh. I. 2nd Tr. E.	7 14 7 26	10 17·7
Sat	29			1st Tr. I. 1st Sh. I.	6 54 8 11	10 13·3
Sun	30	10 56 17 25	Occultation reappearance of A Geminorum (5½) Near approach of B.A.C. 2514 (6½)	1st Ec. R.	7 42 56	10 9·0
OCT. Mon	1	14 24	Near approach of η Cancri (6)	3rd Oc. D.	8 41	10 4·7

THE PLANETS FOR SEPTEMBER.

At Transit over the Meridian of Greenwich.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	12 18 51	S. 4 45½	7″·3	1 35·6
	9th	12 37 32	S. 7 55½	8″·3	1 22·8
	17th	12 38 2	S. 8 27½	9″·5	0 51·8
	25th	12 15 48 12 12 8	S. 4 55 S. 4 14	10″·3	23 50·6
Venus ...	1st	14 54 24	S. 17 42	14″·1	1 55·6
	9th	15 32 46	S. 20 38½	14″·7	1 59·2
	17th	16 12 15	S. 23 2	15″·5	2 3·4
	25th	16 52 37	S. 24 48	16″·5	2 8·4
Mars ...	1st	23 12 59	S. 11 53	29″·5	12 27·9
	9th	23 4 22	S. 12 27	29″·2	11 47·9
	17th	22 56 23	S. 12 45½	28″·2	11 8·5
	25th	22 50 23	S. 12 44	26″·8	10 31·0
Jupiter ...	1st	17 33 56	S. 23 13	37″·9	6 49·8
	9th	17 35 39	S. 23 15	36″·9	6 20·1
	17th	17 38 11	S. 23 17½	36″·1	5 51·1
	25th	17 41 29	S. 23 20	34″·3	5 23·0
Saturn ...	1st	23 17 38	S. 7 0	17″·4	12 32·5
	9th	23 15 23	S. 7 15	17″·4	11 58·9
	17th	23 13 8	S. 7 29½	17″·4	11 25·2
	25th	23 10 57	S. 7 43	17″·2	10 51·5
Neptune ...	10th	2 22 14	N. 12 14½	...	15 1·2
	22nd	2 21 23	N. 12 9½	...	14 13·1

Mercury is too near the sun to be well observed, setting at the beginning of the month about half-an-hour after the sun, the interval decreasing.

Venus sets about an hour after sunset throughout the month.

Mars rises about 40 minutes after sunset on the 1st, the interval decreasing. From the middle of the month he rises before sunset. He is at opposition to the sun on the 5th.

Jupiter may be observed till an hour before midnight, the interval increasing, setting about 9 p.m. on the last day. He is at quadrature with the sun on the 17th.

Saturn may be observed soon after sunset on the 1st. He then rises earlier each day. He will be at opposition to the sun on the 9th.

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF JUPITER.

Greenwich Noon.	Angle of position of ♃'s axis.	Longitude of ♃'s central meridian.		Latitude of Earth above ♃'s equator.	Equatorial diameter.	Defect of illumination.
1877.	°	°	°	°	″	′
Sept. 1	1·93	267·1	870·5		40·10	0·35
—						
2	1·91	57·6	·4	—2·15	39·98	0·35
3	1·89	208·0	·5		39·86	·35
4	1·87	358·5	·5		39·74	·36
5	1·85	149·0	·5		39·62	

6	1·82	299·5	·5		39·50	
7	1·80	90·0	·4		39·38	
8	1·77	240·4	·5		39·26	
—						
9	1·75	30·9	870·4	—2·13	39·14	0·36
10	1·72	181·3	·5		39·02	
11	1·69	331·8	·5		38·90	
12	1·66	122·3	·4		38·78	
13	1.63	272·7	·5		38·66	
14	1·59	63·2	·4		38·54	
15	1·56	213·6	·4		38·42	
—						
16	1·52	4·0	870·5	—2·11	38·31	0·36
17	1·48	154·5	·4		38·20	·36
18	1·44	304·9	·5		38·08	·35
19	1·40	95·4	·4		37·97	·35
20	1·36	245·8	·4		37·86	·35
21	1·32	36·2	·5		37·75	·35
22	1·27	186·7	·4		37·64	·35
—						
23	1·23	337·1	870·4	—2·09	37·53	·35
24	1·18	127·5	·4		37·42	·34
25	1·13	277·9	·4		37·31	·34
26	1·08	68·3	·5		37·20	·34
27	1·03	218·8	·4		37·10	·34
28	0·98	9·2	·4		36·99	·33
29	0·93	159·6	·4		36·88	·33
—						
30	0·88	310·0	·4	—2·06	36·78	0·33
Oct. 1	0·82	100·4	870·4		36·68	·32
2	0·76	250·8			36·58	·32

A. M.

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF MARS.

Greenwich Noon.	Areographical longitude of the centre of ♂'s disc.	‖	latitude	Angle of position of southern pole.	Diameter of disc.	Amount and position of greatest defect of illumination.	
1877.	°	°	°	°	″	″	°
Sept. 1	210·5	351·2	—22·43	159·48	24·76	0·07	295·2
—							
2	201·7	·2	—22·44	159·64	24·76		
3	192·9	·2	22·46	159·81	24·76	·05	310·5
4	182·1	·2	22·47	159·98	24·75		
5	175·3	·2	22·49	160·15	24·73	·04	331·4
6	166·5	·1	22·51	160·32	24·70		
7	157·6	·2	22·54	160·50	24·66	·04	354·2
8	148·8	·2	22·57	160·67	24·62		
—							
9	140·0	351·2	—22·60	160·84	24·56	·05	13·2
10	131·2	·2	22·63	161·01	24·49		
11	122·4	·1	22·67	161·17	24·41	·08	26·5
12	113·5	·2	22·70	161·34	24·32		
13	104·7	·1	22·74	161·50	24·23	·12	35·5
14	95·8	·2	22·78	161·66	24·13		
15	87·0	·1	22·83	161·81	24·02	·16	41·8
—							

16	78·1	351·2	—22·87	161·96	23·90		
17	69·3	·1	22·92	162·10	23·78	·21	46·3
18	60·4	·1	22·97	162·24	23·65		
19	51·5	·1	23·02	162·38	23·52	·27	49·7
20	42·6	·1	23·07	162·51	23·37		
21	33·7	·1	23·13	162·63	23·22	·34	52·4
22	24·8	·0	23·19	162·75	23·07		
—							
23	15·8	351·1	—23·25	162·86	22·91	·41	54·5
24	6·9	·0	23·31	162·96	22·74		
25	357·9	·1	23·38	163·06	22·57	·49	56·2
26	349·0	·0	23·44	163·15	22·40		
27	340·0	351·0	23·51	163·23	22·22	·57	57·7
28	331·0	350·9	23·58	163·30	22·04		
29	321·9	351·0	23·65	163·37	21·85	·64	58·9
—							
30	312·9	·0	23·72	163·42	21·66		
Oct. 1	303·9		23·80	163·47	21·47	0·72	59·9

A. M.

MOON.

Geocentric Libration.

(Continued from p. 200.)

Greenwich midnight. 1877.	in long. λ_0	in lat. β_0
	°	°
Sept. 7	+2·42	+2·39
8	4·17	3·96
—		
9	+5·64	+5·23
10	6·64	6·14
11	7·41	6·66
12	7·63	6·80
13	7·42	6·58
14	6·83	6·05
15	5·93	5·26
—		
16	+4·79	+4·24
17	3·47	3·05
18	2·08	1.74
19	+0·66	+0·36
20	—0·72	—1·04
21	2·01	2·40
22	3·17	3·67
—		
	in long. λ_0	in lat. β
	°	°
Sept. 23	—4·17	—4·78
24	4·98	5·69
25	5·59	6·34
26	5·97	6·68
27	6·12	6·70
28	6·01	6·35
29	5·64	5·66
—		
30	—5·00	—4·62
Oct. 1	4·07	3·29
2	2·88	1·72
3	—1·46	—0·08
4	+0·12	+1·71
5	1·78	3·32
6	3·39	4·69
—		
7	+4·82	+5·74
8	5·94	6·41

On Sept. 22, the day of full moon, and perhaps already in the preceding night, the flattenings or depressions alluded to on page 200, will come into view. They will be fully visible and observable in the following lunation, on Oct. 19, 20, 21. A. M.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To June, 1877.

Lawton, W.

To Sept., 1877.

Lewis, H. K.

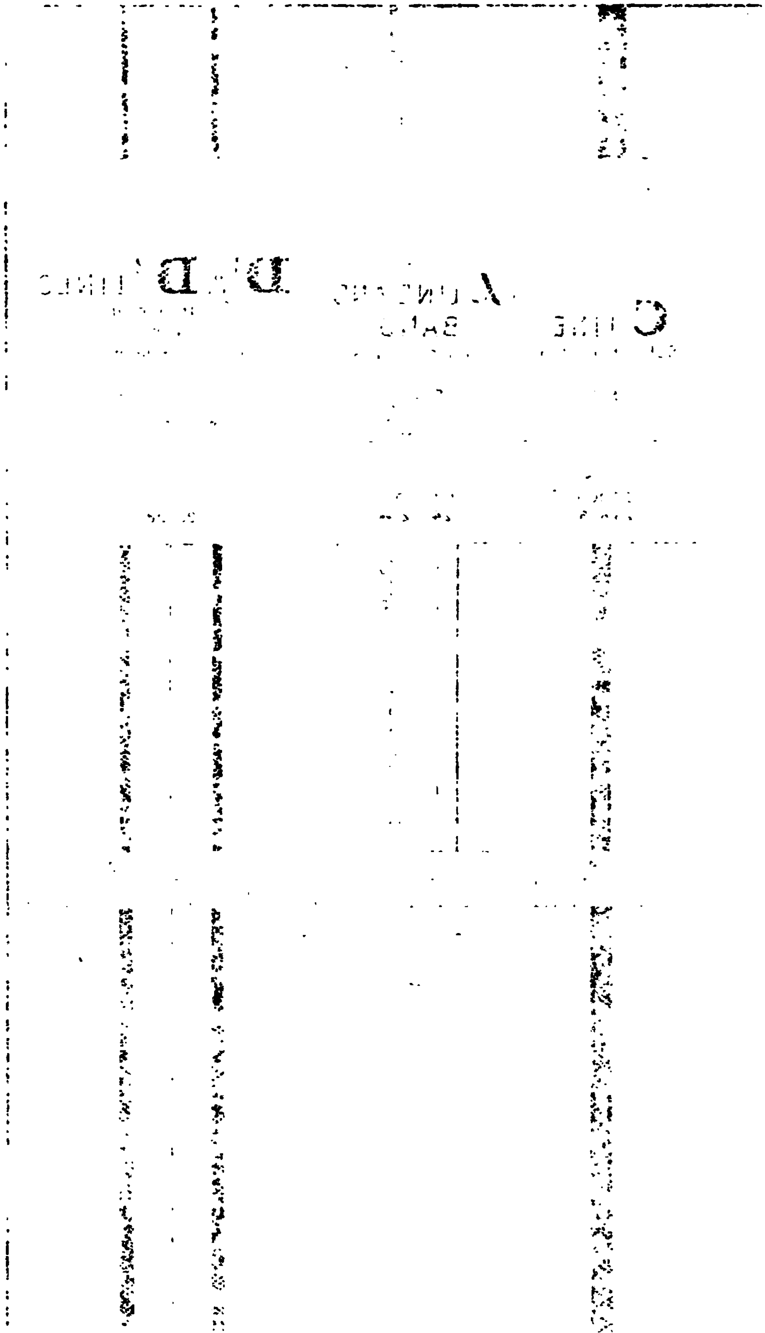
To Dec. 1877.

Almond, G. S.
Hederstedt, H. B.
Metcalfe, Rev. W. R.
Readhouse, Mrs. C.

The Astronomical Register is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings** per Quarter, *payable in advance*, by postage stamps or otherwise.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. Jackson, *Clarence Road, Clapton, E.*, **not later than the 15th of the Month.**

CORAL LINES AS SEEN IN A HIGH SUN



The Astronomical Register.

No. 178. OCTOBER. 1877.

DOUBLE-STAR ASTRONOMY.

At the May meeting of the Royal Astronomical Society in 1874 the following remarks were made by the Astronomer-Royal: "Some few years ago there seemed to be in England an excess in the numbers of observers and measurers of double stars, but a reaction seemed to have taken place, and perhaps it had gone too far; there was no lack of instruments in this country with amply sufficient aperture for the work, but now, at all events, they do not seem to be used. Possibly such instruments are employed in wandering after the new objects that present themselves from time to time, but it would be much better if they were devoted to some systematic work of this kind."

Of the truth of these words there can be no doubt, although an examination of the *Memoirs* alone gives the following list of papers on the subject during the ten years preceding 1874:

1866—"A synopsis of all Sir William Herschel's micrometrical measurements," &c., by Sir John Herschel.

1867—Dawes' great catalogue.

1870—"Seventh catalogue of double stars observed at Slough," &c.

And in 1874 appeared the great catalogue of double stars by Sir John Herschel.

If we go back another ten years we find the following papers:

1854—Powell on the orbit of α Centauri.

1856—Powell's first set of measures at Madras.

1858—Jacob's measures of 120 double stars.

1860—Lord Wrottesley's measures.

1862—Powell's second series of measures at Madras.

During these years numerous papers on double star orbits, methods of computing such orbits, and lists of measures appeared in the pages of the *Astronomische Nachrichten* and other journals. In spite of all these evidences of activity, however, the words of Sir George Airy correctly represented the state of this branch of astronomy. The widespread enthusiasm created by the Herschels and Struve, and maintained by Dawes and Smyth, had passed away. A reaction had set in. But even while these words were being spoken, several amateur observers in widely separated countries, and unknown to each other, were carrying out schemes which, if completed, will do much to wipe away the reproach. We propose, therefore, to give some of the results of careful enquiry, which seem to justify the statement just made.

We need hardly remind our readers that probably every year some careful measurements of double stars are made at most of the public observatories in Europe and America. Lists of measures of important pairs will be found in the volumes issued from Greenwich, Oxford, Berlin, Königsberg, Washington, Cambridge, Cincinnati, and other observatories. More especially is this true of the observatories at Poulkowa and Lund. At the former very considerable attention has always been paid to double stars, and it is hoped that ere many months have passed there will be in the hands of astronomers generally two volumes of measures, the results of more than thirty years' work, by the distinguished director, Otto Struve. The first volume will contain all the measures of the most important of Σ's and ℌ's stars, while the second will give the results of repeated examinations of the pairs discovered by O. S. himself.

At Lund, double stars have received much careful attention, resulting in the publication in 1876 of a fine memoir by Dunér. This work gives the author's measures of a large number of pairs from 1867 to 1876, and is a very valuable contribution to double star astronomy.

At the new physical observatory at Oxford, too, special attention is bestowed on a selected number of binary stars. These are carefully measured, and the orbits of several have been submitted to elaborate investigation.

The double star work of the Cincinnati observatory deserves notice here. In 1875-6 many careful measurements of 166 double and triple stars were made with the 11-inch refractor, and these have lately been published in a separate form. How greatly the separate publication and early liberal distribution of this kind of work by our public observatories would facilitate the labours of computers of double star orbits and others who interest themselves in this branch it is well worth while pointing out in this place.

But the Astronomer-Royal's remarks referred rather to amateur observers than to the scant attention which can be spared for such work in a public observatory, and therefore to this class of workers we will confine the rest of our remarks.

Beyond all question the most honoured place belongs to Baron Dembowski. This distinguished observer began his noble series of measures in 1855; his most recent list appeared in the *Astronomische Nachrichten* in January, 1876. Long may he be spared to enrich double star astronomy with results which for excellence are surpassed by none.

Mr. Knott, who has made a series of very fine measures of binary stars, has of late been occupied in collecting them and throwing them into a form suitable for publication in the *Memoirs*. We trust soon to see them, and also in the future many more measures by this excellent observer.

Mr. S. W. Burnham, of Chicago, in 1873, communicated a paper to the *Notices* of the Royal Astronomical Society, entitled "Catalogue of 81 double stars, discovered with a 6-inch Alvan-Clark refractor." This was the first of a numerous list of papers by this most energetic and successful observer. Possessed of a very fine eye and an excellent 6-inch Alvan-Clark refractor, Mr. Burnham has since 1870 discovered and measured nearly 500 new pairs. That these objects, many of which are extremely close and difficult, will yield a considerable number of most interesting systems, there can be no doubt, and it is with great pleasure that we notice the fact that Dembowski has already executed the measurement of many of them. But this is not all. Mr. Burnham has also found time to collect a vast number of measures of double stars, to study the nature of the changes in angle and distance of many, to classify them—in short, to compile a *General Catalogue of Double Stars*, in which the R.A. and Dec. will be reduced to 1880. This most valuable work will it is hoped be ready for distribution before the end of this year.

Dr. Doberck, of Mr. Cooper's observatory, Collooney, Ireland, has been engaged since 1875 in the study of the orbits of binary stars, and in the actual measurement of such objects. We have no space for the already long list of admirable papers this astronomer has in two years communicated to the *Astronomische Nachrichten*, the *Notices*, the Transactions of the Royal Irish Academy, &c.

In 1871, Mr. Wilson and Mr. Seabroke began a series of double star measures with the Temple equatorial, a fine instrument by Alvan Clark, aperture 8¼ inches. Considering the nature of the daily occupation of these gentlemen, and the fact that the instrument was intended for the use of the school, work of a systematic character and of general interest was hardly to be expected.

Instead of this, however, a valuable series of micrometrical measurements of double stars and several papers on the orbits of binary stars have already appeared in the *Memoirs* and *Notices*. We see, too, that a second series of measures will form part of the next volume of the *Memoirs*.

Mr. Talmage, of Mr. Barclay's observatory, Leyton, has for many years occupied himself much with the measurement of double stars. Three volumes have already issued from this observatory, and we may expect a fourth shortly.

As early as 1869 Mr. Edward Crossley and his assistant, Mr. J. Gledhill, turned their attention to this branch of astronomy. In conjunction with the Rugby Observatory they devised a systematic plan of operations. This included the repeated measurement of all known binary and other important pairs, the formation of a list of binaries, the writing of the history of all the most important objects, and the collection of complete lists of measures. As the years passed on the plan came to include also descriptions of all the equatorials hitherto used in this class of work, a list of all known papers on the subject, and also some account of the graphical and other methods of obtaining the elements of the orbit of a double star. A list of micrometrical measures of 484 double stars, made at Mr. Crossley's observatory has already appeared. Mr. Wilson and Mr. Gledhill have also lately published "A preliminary list of binary and other interesting double stars." This last list will again appear, we understand, as complete as labour and criticism can make it, under the title, "A working list of binary and other interesting double stars." This list if freely distributed will no doubt be most useful to amateur observers. We understand that the whole of the original plan has been carried out, and that the work will probably be in the printer's hands before the end of the year.

Lastly, we must notice the unwearied labours of M. Flammarion in this department. This enthusiastic astronomer has collected very extensive lists of measures of a very large number of double and triple stars, has discussed the apparent or real changes with the greatest care, and has now ready for the press, as one result of his industry, a very complete catalogue of such objects with lists measures, remarks, elements of orbits, &c. This work is but the latest of his researches in double star astronomy, and we have lately heard with much pleasure that M. Flammarion has joined the ranks of amateur observers of double stars, one of the instruments of the Paris Observatory having been placed at his service.

No doubt the above sketch is far from correctly representing the full activity existing in the department of double star astronomy. No doubt both in England and abroad there are many observers quietly but earnestly working and collecting results, which will gradually find their way into the transactions of our learned bodies. Enough, it is hoped, has been written to prove that this branch of astronomy has not lost the charms which long years ago fascinated Struve and Smyth.

OPTICAL SPECTROSCOPY OF THE RED END OF THE SOLAR SPECTRUM AT A HIGH ALTITUDE IN THE SKY.

PART II.—THE TRIAL OF CINTRA.

"The Burling Rocks," said the chief officer of the "Aconcagua," pointing to a group of wild cliffs and rocky pinnacles rising apparently out of the depths of the Atlantic, and in fierce combat with both winds and waves; "they are just five hours' steaming from Lisbon." From Lisbon! But squalls of rain soon hid them from our sight, and drove us pell-mell below, to discuss there how to proceed on landing in that desired Lusitanian city, in order to obtain the best solar spectroscopy; for, though we had received confident advice to try the Braganza Hotel therein, it was accompanied with fear of smoke in any large city of the present age of the world.

"There is no smoke in Lisbon," slowly and sententiously remarked a tall Englishman, who had been there already two summers, and was now on his way, with his wife, rejoicing to try it for a third season. Very soon too after that the squalls died away, the rain ceased, the clouds heaved off the scene in one immense canopy, and we were entering the mouth of the mighty Tagus; its fabled golden sands and green-yellow cliffed hills to the south; the serrated range of the dark blue mountains of Cintra far away to the north; and onwards towards the east, the river's northern bank, or rather coast line, so gallantly broad is the noble stream, fringed with palaces and castles, convents now convents no more, and white houses of every degree. The numbers of these continually increased, clustering thicker and thicker the further we proceeded amongst shipping of all nations, and plenty of room for them all and many more too, until at last, white stoned, and seated on more than seven hills, this must be Lisbon which is glittering in light and glory before us. So it was; and who could have admired it more than we did, until, ah! that luckless moment, until examining with rather more critical eyes than our neighbours, and following the wind as it breathed over the city from the north-west, and died away to the south-east,—yes, out there lay the evil thing we had fled from our own country to avoid; out there sat the fiend mocking at all our spectroscopic hopes; for in the tint of that atmosphere which had passed over the city was nothing less than optical poison; and, faint as was the hue, yet it spoke unmistakably to manufacturing coal smoke; or in one word "the trail of the serpent was over it all."

But the waters were sparkling immediately around us, the agent's little tug-steamer graciously conveyed my wife and self to shore; and there, most kindly awaiting us, with heartiest welcome, was our old friend of Pulkowa Observatory reminiscences, Mr. F. A. Oom, the Astronomer-Royal of Portugal. He had come that afternoon from his observatory several miles away for this very purpose; had obtained particular permits from his Government to let all our scientific packages pass the Custom-house free; and he did not leave us in the late twilight until he had seen us fairly established in the Braganza Hotel; close to the Tagus, and yet so high above it, that surely there the atmosphere will be grandly clear.

Heavens! what a scene the next morning! The lake-broad river, the bright coloured cliffs, five, seven, and even ten miles away, reflecting themselves brilliantly in the placid tide; the sky of cobalt blue, or flecked only with a few detached white clouds; the temperature up to 70°; the radiation such that everyone in the streets carries an umbrella, and hardly a beggar is without his parasol. Protected from the sun by a long balcony overhead, we gazed out with joy on this exciting panorama of the south; but, almost immediately from one of the chimneys of houses right underneath the imposing facade of the Braganza (chimneys that do not look like chimneys, ornamental blocks or pillars, or scrolls of white masonry with a slit in the top like a batswing gas burner on a large scale), up came volumes of blue smoke into our eyes and noses, diffusing itself through the air and scattering the sun-light far and wide. A little longer and another household chimney was at work, and then another and another.

Oh! those are not fires for warming, they told us, only for cooking food. Well, let it be so; but they make smoke, and fill the air for a time with finely divided particles of solid matter; and as each house has several times a day its cooking fire alight, and as such houses by the thousand are spread over several square miles all round and about the Braganza Hotel, except towards the river where the tale is taken up again by the steamers continually puffing to and fro—the winds from every quarter are always bringing from far or near someone's smoke over that devoted balcony; while more terrible still, and as we learned too well by the second day's experience, there is a huge red brick chimney only a few hundred yards to the west; a chimney of enormous capacity, and which both by day and by night, both in early morning twilight and under the silvery moon, is *always* throwing out portentous volumes of black-brown smoke, fitted to shame Newcastle itself.

It was the chimney of the gas works we were informed, and under an English engineer. If we had been King of Portugal for only a quarter of an hour, would we not immediately, in the interests of all astronomers, all spectroscopists, and the whole photographic world—would we not have taxed both those works and that engineer according to the number of cubic feet of the foul fuliginous cloud they emitted through every twenty-four hours. But as we were no such royal personage, on the third day of our new experiences we turned from the hopeless scene, and taking carriage drove out to inspect the situation of Cintra, sixteen miles away to the north; where, amongst quintas, or country seats, and bowery gardens and hill-sides densely covered with trees, and a sparse population still living in old-world style, altogether innocent of coals and gas, manufactures and railways (the celebrated tramway bubble excepted, and even that is now in process of being taken up and sold for old iron), we might hope to see the sun as nature originally intended man to see it, through air more than crystal clear.

A bright, a burning day it was on that Cintra road; and in Cintra itself the high sun darted down with vivid beams through all the deep dells and winding paths, making any attempt at walking about an affair of dodging from side to side of the road, wherever a tall wall, or big rock, or spiry tree threw a narrow shade. But the extensive gardens and rustic life out there were undeniable. An appropriate hotel, too, was found on a wooded slope well calculated to filter the lower air; so prospective arrangements were entered into with its proprietor, and when we returned to the Braganza in the cool of the evening after this apparently successful reconnoissance, the spectroscope boxes were packed into two primitive looking carts, whose honest, ancient drivers undertook to start at break of day, and to deliver the packages safely at the Cintra Hotel before dark in the evening, as well as to report themselves in Lisbon after another day.

That day was Sunday, when we ourselves rested according to the commandment, or rather desired to do so; but it was also the fête of St. John, and he, if Portuguese tradition says true, delights in nothing so much as bonfires and popular open-air demonstrations. Lisbon was therefore in a most excited state all through the brilliant day and blazing midnight too; while lottery tickets were egregiously occupying all ranks of people on one side, and bull fights on another.

Monday, however, came at last, and with it our excellent friend Mr. Oom, desiring to see us once more before we left the Braganza. Its smoke he allowed had grieved him, and he approved of all our proceedings to try to get out of its range; "Yet," added he most kindly, "if Cintra also *should* fail, remember you have still the Royal Observatory of Lisbon to fall back upon; for I have obtained leave from the Minister of the Interior for you to put up your instrument there as you like, and for as long as you may find convenient."

Our start with bag and baggage that day on the Cintra road once more was purposely delayed until the meridian heat was passed; and then, as the attentive hotel keeper accompanied us to the carriage, we asked him "Do you think it will rain to-day?"

He looked up at the sky, and saw the thick cirrus haze which was beginning to spread over it, but laughed outright at the idea of rain at that season; "It couldn't be!" Very well, we replied to ourselves, we make a note of that as native opinion, and put it down under our other note that at 7h. a.m. this morning the rain-band in the sky spectrum, always strong in Lisbon, has suddenly doubled in intensity. Before, too, we were five miles on the journey, rain did begin to fall, and kept on almost up to Cintra itself. The rain-band prediction then was true again; but yet I will by no means pretend that we had that evening any idea of *all* that was in store for us at the delightful country quarter we had then reached; a locality some 5 or 600 feet above the sea level, open generally towards the north, but closed by the hills in all other directions.

What a scarlet streak it was above the sea-horizon to the north-west after sunset that evening; and what a picturesque, if cloudy, sunrise gilded the north-east next morning; and our bedroom window, looking out due north, commanded them both at that season (June 26). Oh! the reds, the oranges, the violets and the lovely dove greys which immediately preceded the rising of the sun; shining here, glowing there, either all over the lightly clouded sky, or if most strong in the east, reflected also from the west; and the forms of the clouds and cloudlets and groupings of pellets of vapour carrying these tints delightful, refined, multitudinous (the very words noted down at the time),—but, and this was also noted then and there,—*the spectrum rain-band exceedingly strong!*

At actual sun-rise, about 4h. 40m., a.m., the solar spectrum, also looked at, was ominous indeed. The regular rain-band near

D, all but black; another rain-band following, or on the yellow side of C, very dark too; a thick rain line between *b* and F, very black; a strong rain line on the green side of the δ band; but little *a*, so-called, transcending everything else in blackness and positive majesty of breadth. At sunset that evening the note was, "Rain-band on setting sun blacker than we have ever seen it before."

Well, but, says a reader, pray what were you doing all that day, between sunrise and sunset, your first whole day of being at Cintra with the large spectroscope boxes to your hand?

This: we engaged an upper chamber on the opposite, or southern, side of the house, and there the instruments were unpacked, and set up in position. The heliostat, fitted to a long platform traversing over two tripods, was enabled to be protruded at pleasure through the window to bring in special rays, while black curtains excluded all other light; and the spectroscope, with its various adaptations, was mounted on another tripod and put through its facings; but all in dumb show, for no more of the sun was seen after that early glimpse at sunrise. So in the afternoon we drove off to see the mansion and gardens of Vathek, now belonging to Viscount Montserrati, otherwise Mr. Cooke; a mansion which is the Alhambra classicized, and also adapted to modern civilized life and intellectual existence; while the garden exhibits a wealth of flowering shrubs and remarkable trees, collected from the sub-tropical regions of three quarters of the globe: after carefully studying which, we returned to the hotel through gently mizzling rain.

But what torrentuous rain followed that night, with thunder and lightning, flash answering flash from opposite sides of the horizon and sometimes half-a-dozen vivid jagged streaks appearing at once, while every road became a cataract, and the hill—though crowned on its summit with monster boulders of the hardest description, yet consisting elsewhere of a porous, friable, decaying yellow granite, overlaid with tangled vegetation—became utterly soaked with a plethora of water. But water at a temperature of 70°, so next day and the day after, and the day after that, the whole region was in a steamy fog of its own creating; and everything, both in doors and out of doors, remained damp, clammy, disagreeable, from morning to evening. At length with Friday night some clear sky appeared; but after that came the festival of St. Peter, who, if Portuguese ideas are correct and wrong not the holy man, delights in nothing so much as squibs, rockets and crackers; wherefore it was nothing but fiz, pop, bang, —bang, bang, bang,—all the livelong night, and most of the days too, while the little country inn was crowded to overflowing with hilarious townspeople, come to assist at the hubbub and general noise.

All this time we ourselves had a quiet, suggestive prospect close under our northern window, of the quaint old palace of the early kings of Portugal; the Alhambra of their land, for it was begun by the Moors soon after their conquest of the country in 710 A.D., and was occupied by them until their expulsion in 1178 A.D. Then came the mediæval Christian kings of Portugal, each adding his quota of strange architecture: and the custodians tell of each room what surpassing crimes were perpetrated, sometimes striking virtues exhibited, there; but above all they delight to show the kitchen of Queen Philippa's time, with its two enormous conical chimneys; or rather these show themselves outside as the chief architectural feature of the whole pile. Such chimneys—broader below than a modern steam-engine chimney; and if not so high, yet at least much higher than an ordinary blast furnace, and an excellent idea too for ensuring comfort and efficiency in the first requisite of every inhabited house: an idea which apparently Queen Philippa (sister of John of Gaunt, Duke of Lancaster, and flourishing here as the honoured wife of king João the 1st, from 1387 to 1415) may have brought with her from her brother's manufacturing county, when Manchester and all its present tall chimneys were still in embryo.

And again, in the extreme north-eastern distance, 20 miles away, were just visible in the early morning light the towers of the far larger, far more architectural, but very modern and wasteful palace, the palace of Mafra: the largest and least required palace of the whole world, but which the king of the time, still known as "O Magnifico," would insist on building, in order to carry out to the uttermost a hasty vow, far more egotistical than religious. And we could on those densely clouded days walk in the little town of Cintra, amongst the dogs who live night and day in the chief square of the town, like the pariah dogs of eastern cities, but without any of their ferocity; and we take some coppers to the poor prisoners in the jail, who are allowed barred windows, like wild beast dens, on the level of the street, whence they may thrust forth their hands and clutch alms from every passenger; or we might climb the hill and roam to our heart's content there, in King Dom Ferdinand's park and garden of rocks and Moorish ruins, delightful plants and strange monastic architecture—but what was all this to satisfy minds which were burning with desire to spectroscope the sun, the sun himself, flaming, hot and clear? A week had now passed away at supposed delectable Cintra, and rain, steam, fog, and cloud formed almost the only weather there. However, we must not give it up too soon, for this sort of thing cannot prevail at Cintra for ever, or how could the gardens be so gay with such wealth of scarlet and purple flowers.

The second week at Cintra opened happily a little better, for the wind began to blow from the north-east, and the air, at last, gradually dried. On July 3rd, too, a spectrum observed in the small way at sunrise showed little *a* not much stronger than great A: so that was immediately seized on as a happy indication of further dry, and not rainy, weather coming.

Such, moreover, was the case, but still not clear weather for Cintra. A sort of north-east, or trade-wind, clouds, were driving all day across the sky, and however small they first appeared in the distance, they always grew larger and larger as they approached Cintra; not from mere perspective but from prodigious increase of the cloud itself. Day after day the distant country seemed to be enjoying clear sunshine, but immediately round about and above Cintra, and not very high above the boulders and Moorish towers of its hill-tops and the Penah Palace there, the original of Byron's "horrid crags with toppling convents crowned," was a constant cloud, stationary on the whole, but not in its particles; for, like the so-called Table-cloth on Table Mountain in South Africa, it is formed successively in each molecule of moist air, as this rushes over the mountain top; but is again re-dissolved and disappears on the other side after passing that cooling region.

Occasionally indeed this local cloud about mid-day would be so thin that the high sun was just visible from our observing chamber; *was* observed, too, with the large spectroscope; and not altogether badly, though through rushing, driving mists in the upper air all the time: until, in a few minutes more, the clouds thickened and there was an end of the sun for that day; while we noted with vexation that they were exactly the same sort of clouds as we used to have on the Peak of Teneriffe daily, but there beneath our feet instead of as now above our heads; and if we could only have been elevated at Cintra some 5,000 feet vertical, not one of those distressing masses of mist would have interfered between us and the sun.

By dint, however, of watching for opportunities of these clouds thinning for awhile, a good deal of useful practice with the large spectroscope was being obtained, and one of its first employments was, to enable us to look a little deeper into the rain-band of the waistcoat pocket spectroscope, so often alluded to.

The large spectroscope should rather have been termed a spectroscope of degrees; for whereas *size,* though good in some things, is not very particularly either here or there in spectroscopy, the amount of *dispersion* is everything: and dispersion is really so extraordinary an effect in nature, so utterly diverse from mere visibility, mere optical magnifying to any extent, that I could not rest until my chief spectroscope was enabled to ring a whole series of dispersion effects. Hence, while the collimator and telescope were each about 17 inches long and 2·3 inches in diameter, and the eye-piece of the latter such as gave a magnifying power of 10 times, and these things, not very first-rate perhaps in quality, were a constant dish throughout, there was a series of 6 prisms, as follows—each one of such prisms being arranged for use by itself with equal convenience in the apparatus; but when so used giving these different amounts of dispersion, *i.e.*, of length to the solar spectrum from its A, to its H, line:—

Prism 1 = 0°·7
" 2 = 1·2
" 3 = 3·3
" 4 ≐ 6·7
" 5 = 13·4
" 6 = 32·0

Hence on looking into the apparatus with prism 1 in place, and turned to the sky-light merely, we see the whole length of the spectrum abundantly in the field of vision; and even with a narrow slit, the colours of said spectrum are vivid in the extreme; faint nebulous bands across the spectrum, if present, are strongly marked, but the celebrated Fraunhofer lines are few and barely visible.

Prism 2 shows a larger spectrum, a little more of the Fraunhofer lines, less of the brilliancy of colours, but the bands remarkably well.

Prism 3 exhibits a wonderful increase in the number and strength of the lines, less of the bands, and less of the brilliancy of colours: and so the prism series goes on, up to prism 6; every successive number continually decreasing the vividness of colour, increasing the number and distinctness of lines, and decreasing the visibility of bands or nebulous bars across the spectrum; until with that final prism of the set, No. 6, colour is next to nowhere, and the angular place of a line in the spectrum is almost the only phenomenon left for philosophers with powerful spectroscopes to dispute about.

Now let us apply these respective features to the rain-band, when a rain-band is to be seen, in the light of the sky.

With prism 1 the first effect is merely that the D line (in itself showing salt burning in the sun) apppears rather thicker than usual; dirtier, too, like a dusty spider line; and by and by, if the natural rain-band increases in strength, that adventitious dirtiness of the D line extends itself a little on the red side of D, so as to form something of a shade up and down on that red side of it, but still a shade only two or three hairbreadths broad.

With prism 2 the rain-band is seen as a further increase of improper thickness and even scandalous dirtiness of the D line (though as produced by solar chemistry and the mechanics of the spectroscope, the D line is one of the cleanest and most exquisite things in the world); the shade on the red side of D is now broader, and in that shade, running up and down it parallel with the grand and orthodox D line, may be just perceived an almost infinitely fine line, and if the air is very moist and warm, two such extra lines.

With prism 3 the scene almost entirely changes, for though the D line is still dirty enough, the shade-band on its red side is no longer the chief feature; but in its place the two fine lines, only barely visible before, have now become strong, distinct and separate lines, the outermost considerably the stronger of the two; but they are both dirty, brownish, hazy lines, like worsted thread compared to taut silver wires. There is a similar faint, hazy, worstedy line on the yellow side of D, half-way between it and the broad nebulous band of δ (see, for δ, the plate in *Astronomical Register* for September). But while the rain-band lines vary in intensity with the amount of moisture in the atmosphere, and its temperature (always supposing that the sky light is strong and clear to show them) the faint line or streak on the yellow side of D never varies. It is in fact a group of very thin solar lines, but so close together, that prism 3 not being able to discriminate each individually, they have altogether just as hazy, dirty, powdery a look as any of the rain-band lines themselves, thus far.

Now take prism 4. We are here beginning to use so much glass that unless the daylight is very strong this prism will show us no more detail than prism 3; for it will merely give a larger, wider, fuzzier, and still more dirty look to both D and the other lines, streaks, and bands. But let only the light be strong, and instantly the observer may exclaim, with wonderment of admiration, Why, there is D double! Yes, even solar D, neatly composed of two hard black lines laid close side by side, and distinguishable instantly by their physical constitution, or their jetty look, from all the dirty brown shade behind and about them connected with the rain-band lines; these latter lines, too, are no longer seen as single, even of worsted, but as possibly composed of a congeries of finer lines.

Wherefore we next try prism 5, and find the solar D lines herein not with merely a narrow cleft between them, but separated by more than the diameter of either; the old rain-band worstedy lines are now certainly to be made out as bundles of finer lines; and some hazy shadow beyond them is also beginning to arrange itself into lines as neat and measurable as any of the others.

So finally we try prism 6, and what a world of information bursts upon us. The solar D lines are now so far one from the other, that we cannot believe there could ever have been any difficulty in perceiving their duplicity, if anything of them at all was seen. But here is the difference between magnifying and dispersion; *magnifying* increases not only the space between the lines, but the breadths of the lines themselves, together with all the errors, smearings and perversions of both the slit and its collimating object-glass sadly, if not fatally; but *dispersion* leaves these things alone, and applies itself almost solely to increase the space *between* spectral lines.

And then to see how this principle acts on the rain-band; its former worsted apologies for lines, and its dirty nebulous bands, shade and haze—why now they are all resolved into lines, sharp, hard lines, too, though some may be thick and some thin, and; there is hardly any shade, any band, matter left in the spectrum at all.

This prism 6 was therefore quite a wonderful affair; and the manner in which it opened out spaces between lines which all the striving of the spirit and straining of the eye with other prisms of less dispersive power could make nothing of, was a gift to poor human nature, a conferring of a new kind of power on the human eye; and, to so strangely exalted a degree; actually setting most of the spectral phenomena on their ultimate basis at once. Wherefore I could never be sufficiently thankful that Mr. Adam Hilger did undertake early last spring to produce that prism 6 with all the dispersion qualities which it was eventually found to possess.

"It is, I firmly believe," wrote Mr. Hilger in his last letter before we left Liverpool, "the best prism (or set, for three principal prisms were concerned in it) that exists in the world, for dispersion combined with light; but sometimes I feel doleful about it, for the rest of your apparatus, in which it is to be placed, is not altogether what it should be; and no man need hope to reach the higher fields of spectroscopy unless, besides good prisms, he has also *splendid object-glasses* and a MAGNIFICENT SLIT."

There was undoubtedly much truth in what this admirable artist in scientific manufacture thus penned in his moment of despondency; and I know not how much better still the solar spectrum may be seen by other observers, happy in being able to afford to have every part of *their* spectroscopes of the best, but as, an example of how some of the more ordinary and well known lines *were* seen with my apparatus, I subjoin a plate of B, C, α and D from our Cintra observations at an altitude of 70°, together with copies of what has been recorded of them both by the admirable *savant* of Upsala, M. Angstrom, and by the Royal Society of London in their second edition of "Hennessey's Himalayan Spectrum in 1875"—when seen also in a high sky.

These old friends, B, C, α and D in the brighter part of the spectrum could be observed well enough through the flying vapour which, even at noon day, formed the zenith sky at Cintra. But we went there to study the comparatively unknown lines in the exceedingly faint ultra red; and how could that be attempted except in a firmament perfectly clear? and were we ever to have such a thing as that in this, by all the world admired, garden seat of Cintra?

So my wife and I walked out one afternoon right away to the open northern country, and sure enough there was sunshine *there;* the roads were dry, the wild plants wiry, the geranium flowers covered with road-side dust; and if a little cloud, no bigger than a man's hand, did appear for a moment in the sky above our heads, it sailed away towards the Cintra hill, expanding portentously as it went, until it joined and locked into the perpetual congeries there. What a poke then we had got our heads into, by trusting to our one day's sunny visitation on June 22nd! We reflected troublously on that; and when on presently returning to Mr. Victor Sassetti's hotel, we found a Scottish acquaintance just arrived there from Lisbon—behold his chief story was of how he had had to wear dark-coloured spectacles to save his eyes from the intense sun during all the earlier part of the journey, but not at all when he got under the Cintra cloud.

That was the last straw to break the back of our resolution to persist in fighting for Cintra's astronomical credit; and had not Mr. Oom so pointedly, as well as kindly, told us, "if Cintra *should* fail you, you have the Royal Observatory of Lisbon to fall back upon"? So the instruments were packed up next day, sent in carts across the country by a roundabout road to the Observatory, in order to avoid going twice through the exacting municipal gates of Lisbon, and we ourselves started for our old quarters in that city on the morning of July 10th.

A cloudy day it still was at Cintra, as we left its vaunted "glorious Eden" behind us; but mile after mile we rejoiced to see how the clouds thinned, the sun shone through, the dust began flying. We became at last covered with fine white dust, like millers; but we accepted it all thankfully as proof of now at last, or again, approaching a real solar observing neighbourhood. The thermometer under such shade as the carriage roof with open window ventilation afforded, rose from 71° to 87° Fah. The geraniums and juicy grasses of Cintra disappeared from the road side. No more now of its "sunken glens, whose sunless shrubs must weep," nor its hoary cork trees waving pendant mosses from every bough; but gaunt aloes stood on arid slopes, and scorched up Australian gum trees with shadeless dangling leaves, looked at us over stony walls; the homeless dogs and equally public cats were sleeping, or lying exhausted, under the eaves of roofs, or wherever a handful of shadow was to be found; and when we reached Lisbon, the fair city seemed positively white-hot in the sunshine, though hazy with eternal smoke.

But at the Royal Observatory, away to the west, we hoped presently to have the sun of Lisbon without the smoke thereof.

P. S.

THE ATMOSPHERE OF THE PLANET JUPITER.

By E. Neison.

(*Continued from page* 230.)

The mathematical formulæ by which these results have been obtained have been given in the *Quarterly Journal of Science* for October, 1875, and no doubt can exist as to their accuracy. The results may be given as follows: If δ, t, be the density and temperature of the atmosphere at the uppermost cloud-bearing strata of the atmosphere which is visible to us, then at any distance s in miles beneath this layer, the density and temperature will be δ and t respectively. And the value of δ may be calculated from the formulæ—

$$\delta = \delta, e^{-u}$$

where u can be calculated by the formulæ.

$$s = \beta^{-1} \left\{ (1 - f)\, u + \frac{1 + r}{r} (1 - c^{-ru}) \right\}$$

In the last equation β is a constant depending on the temperature t_0 at the surface of the planet, and the specific gravity D of the gas constituting the atmosphere, in the last air being taken as unity. Therefore—

$$\beta^{-1} = \mathrm{D}.\, \frac{1 + 0{\cdot}003665\, t_0}{0{\cdot}500}$$

The two factors f and r are constants, on whose values depend the rapidity and degree in which the temperature of the atmosphere decreases as the height above the heated surface of the planet increases. The value of these two factors depends primarily on the temperature of the surface of Jupiter. Thus the value of f will be—

$$f = 1 - \frac{1 + 0{\cdot}003665\, t_2}{1 + 0{\cdot}003665\, t_0}$$

when t_2 is the temperature of the uppermost layers of the atmosphere, that is to say, the temperature of space. It is evident that f must also lie within zero and unity. It cannot be zero, for this would make the temperature of the atmosphere uniform throughout (r really depends on f, for it must vanish with f). The surface of the planet Jupiter, being supposed to be intensely heated, and the temperature of space being known to be much under zero centigrade, it can easily be shown that the value of f must be greater than two-thirds and less than unity. By assuming different values for t_2 and t_0 any different value may be arrived at, and its results tried.

The rate at which the temperature falls depends mainly on the value of r, and its value can be found from the equation—

$$r = \frac{1.}{u_1} \log.\left(\frac{1 + 0{\cdot}003665\, t_1}{1 + 0{\cdot}003665\, t_0} - \frac{1 - f}{f}\right)^{-1}$$

where δ being the density of the absorption at the surface of the planet, the factor u_1 is found from the equation—

$$u_1 = \log.\left(\frac{\delta_0}{\delta_1}\right)^*$$

This last equation is practically of little use, so that r remains to all intents an arbitrary constant. If its value be assumed to be unity, its influence disappears. If it is taken greater than unity, it corresponds to a quicker decrease of temperature, and reduces the depth of atmosphere which can possibly exist for a given temperature at the surface of the planet. If supposed less than unity, it corresponds to a slower decrease of temperature. It will be found that it cannot be assumed to be much less than $\frac{1}{4}$ if the temperature of the planet be supposed to be high, without making the temperature of the upper cloud-bearing strata greater than is consistent with the fact that they are supposed to be the uppermost cloud-bearing strata. Because if it be supposed that the upper cloud-bearing strata of the atmosphere reached a considerable temperature, the clouds would be vaporised, and ascending condense still higher in a cooler region. The new strata would be then the region to which the considerations would apply. The value of r must never, therefore, be made so small that the temperature of the upper cloud-bearing strata is made too great. This means really that this temperature should not differ from 0° centigrade.

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* These formulæ are not the most convenient which could have been contrived for this special purpose. They were put in this form for another end, and it has not been thought necessary to transform them.

The following considerations will show how these formulæ can be applied to the end in view. When δ becomes 1,000 times the density of that of our terrestrial atmosphere, it would to all intents cease to behave as a gas, and become to all intents a liquid. No increase of temperature would of course affect this. It can, therefore, be supposed that at the depth where δ is equal to 1,000 times the density of our own atmosphere—that is when it would be denser than water—the surface of the planet (liquid or solid) would be reached. Next, considering δ, it is probable that no cloud layers would permanently exist if the density of the atmosphere was less than one-tenth of that at the surface of the earth. This is the case on the earth, and would be probably the case on Jupiter. Take, however, an extreme supposition and suppose that clouds would exist on Jupiter in strata of its atmosphere only one-hundredth of the standard density of the terrestrial atmosphere. Putting then $\delta = \frac{1}{100}$ and $\delta = 1000$, and the first equation gives—

$$\frac{1}{100} = 1000\, e^{-u}$$

$$u = \log.\ (100{,}000) = 11{\cdot}515$$

(The logarithms are throughout hyperbolic or natural logarithms.)

Substitute this value in the equation to s and—

$$s = \frac{1 + 0{\cdot}003665\, t_0}{0{\cdot}500} D \times$$

$$\left\{ (1 - f)\, 11{\cdot}515 + \frac{1+r}{r}\left(1 - \left[\frac{1}{100000}\right]^r\right) \right\}$$

By giving different values to D f and r in these formulæ, different permissible atmospheric conditions may be found for the planet Jupiter. It must be remembered that f cannot exceed unity, nor r be so small that $(1 + 0{\cdot}003665\, t_0)\left(1 - f + f\left[\frac{1}{100000}\right]^r\right)$ differs sensibly from unity.

It must be pointed out that to suppose D to be materially less than unity, is equivalent to assuming the atmosphere of Jupiter to be composed of some element or elements entirely unknown. For the only element which possesses a gaseous specific gravity materially less than that of atmospheric air is hydrogen, and it is obviously out of the question to suppose the Jovian atmosphere to consist of incandescent hydrogen. The spectroscope, moreover, has shown that it is impossible for the principal constituent of the atmosphere of Jupiter to be a gas much lighter than atmospheric air. Thus Dr. Vogel and others have shown that the

Jovian spectrum contains dark lines, similar to the ordinary telluric lines, and known to be due principally to aqueous vapour. The light received from Jupiter being solar light reflected from the upper cloud-bearing strata, this shows that aqueous vapour must exist at least in the cloud-bearing strata, and probably above it. In fact the presence of aqueous vapour in the atmosphere of Jupiter shows that probably, nay, almost certainly, that the clouds in the higher regions of the atmosphere of Jupiter are mainly due to aqueous particles, exactly in the same manner as the terrestrial clouds. The proof of this conclusion, like that on many other points connected with the physical condition of the planet, is necessarily primarily a chemical one, and rests on the known influence of aqueous vapour on other chemical compounds. Next the upper portions of the atmosphere of Jupiter must be considered to be in statical equilibrium, that is to say, although, doubtless, subjected to numerous minor disturbances, such as air currents, and to be generally free from any very violent disturbances, like those which occur in the solar atmosphere for example. Under these conditions it is a known physical law; a law which has also been deduced by Clerk Maxwell from the dynamical theory of gases, that in a mixture of different gases, each one so disposes itself that it would be in equilibrium were there no other present. Consequently aqueous vapour would not be found permanently in the atmosphere of Jupiter at any greater height above the surface than it would occupy were it alone to constitute the atmosphere. Suppose then the Jovian atmosphere to contain some hypothetical gas much lighter than aqueous vapour. Then the last conclusions show that this hypothetical constituent of the atmosphere of Jupiter may be neglected in finding the greatest depth beneath the upper cloud-bearing strata that the atmosphere possesses. For it has been shown that the upper cloud-bearing strata of Jupiter's atmosphere permanently contains large quantities of aqueous vapour (were the amount small no absorption lines would be shown by the spectroscope), and this aqueous vapour would not ascend permanently to higher altitudes than it would occupy were it alone to form the atmosphere, consequently the height above the surface of these visible uppermost clouds cannot be greater than what would be their height were the hypothetical gas non-existent, and the atmosphere composed of aqueous vapour only. This conclusion proves that the presence of a hypothetical new gas, much lighter than air, would not disturb the conclusions which have been arrived at with regard to the maximum depth beneath the visible cloud-bearing strata to which the atmosphere may extend. At a subsequent period it will be shown that besides being immaterial it is impossible that the atmosphere of Jupiter can contain such a gas.*

It may, therefore, be taken as established that the planet Jupiter cannot possess an atmosphere which extends to a greater depth than 150 to 200 miles beneath the visible uppermost cloud-bearing strata. It will be found, in fact, that it is only by violating conditions which must be observed, that is to say, by assuming the atmosphere of Jupiter to be in a condition which it is known it either does not or even cannot possess, that any greater depth can be deduced. The formulæ which have been given will prove this. It is probable, however, that the atmosphere of Jupiter does not extend 100 miles beneath the uppermost layer of clouds; yet this depth, small as it looks, is fully ten to fifteen times greater than the corresponding depth in the case of the earth.

Returning to the interpretation which has been put on the phenomenon observed by Mr. Todd, at Adelaide, it remains to see what depth of atmosphere to Jupiter these observation are claimed as proving. Both Mr. Todd and Mr. Ringwood saw the I. satellite of Jupiter projected on the disc of Jupiter, as if at internal contact, so that the whole disc of the satellite was seen within the limb of the planet. That is, the apparent centre of the satellite was rather more than half-a-second of arc within the apparent edge of the planet, the diameter of the first satellite being taken as 1·08″. The image of the satellite not being distorted, necessarily the preceding edge of the satellite must have been rather over one second of an arc distant from the apparent edge of the planet.

Mr. Todd's interpretation of this phenomenon seen by himself, and supposed to be due to the action of an atmosphere, is very simple. His words are very definite. The satellite appeared "as if seen *through* the edge of the planet, as if the latter were surrounded by a transparent atmosphere laden with clouds." The view taken, therefore, was that the phenomenon was exactly what might be expected were the planet a circular disc with a semi-transparent border of rather over one second of an arc in thickness. As the satellite passed behind the transparent border, the satellite was seen through it, and only disappeared when it passed behind the central opaque portion. The phenomenon was, therefore, what, neglecting refraction, might be expected from an atmosphere to Jupiter about one-and-a-half seconds of arc deep, and filled with small clouds. The small clouds must, necessarily, be supposed thin enough to enable a body to be seen through them, and yet bright enough to appear as bright as the remainder of Jupiter.

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* The above applies solely to a gas much lighter than air; therefore the fact that the spectroscope shows lines of unknown origin does not apply, for this substance may be of similar density to air. It has not, however, been shown that the new lines may not owe their origin to the fact that a greater mass of atmosphere is traversed by the solar rays on the planet Jupiter than on the earth.

At the mean distance of Jupiter from the earth, an angle of one second of arc may be taken to be subtended by a length of two thousand miles. It follows, therefore, that Mr. Todd considered the depth of the atmosphere of the planet Jupiter to be about 3,000 miles.

The interpretation to be put on the phenomenon has not been clearly stated by the distinguished astronomer, who has claimed these observations as proof positive of the correctness of Mr. Proctor's view of the constitution of Jupiter, and of the existence of an atmosphere to Jupiter which possesses an immense depth beneath the apparent cloud surface of the planet. It appears to be practically the same as Mr. Todd's. In the articles already referred to, whilst Mr. Todd's observations and views are quoted, and the same conclusions arrived at, the distinguished writer in question neither points out that there is any error in the interpretation of Mr. Todd, nor gives what he considers himself to be the true manner in which these observations prove the existence of a very great depth of atmosphere beneath the visible surface of the cloud. Both these facts point to his adoption of the same view as Mr. Todd.

The view taken of these observations appears, therefore, to be very simple, and as follows:—The satellites were seen whilst approaching occultation to be projected on the disc of the planet. As whilst thus projected they were thought to have appeared fainter than when seen in a corresponding position during transit, it was concluded that they were seen through the limb. As they were seen entirely within the visible edge of the planet, it is considered that the edge of the planet is formed by an atmosphere filled with fine clouds so as to render it semi-transparent. As the III. satellite was seen thus situated, and has a diameter of one-and-a-half seconds, the transparent border or atmosphere must be also one-and-a-half seconds wide. Therefore these observations establish that the planet Jupiter has an atmosphere which extends for one-and-a-half seconds, or 3,000 miles beneath the visible edge of the planet. It is to be remembered, however, that Mr. Todd has not explicitly claimed for these observations the establishing the last portion of the above.

It is possible, now, to answer a portion of the question in the first part of this inquiry. It has been shown that the interpretation which seems to have been put by Mr. Todd and the distinguished astronomer in his papers in the *Spectator* and *Popular Science Review*, on the phenomenon observed during the occulation of his satellites by the planet Jupiter, assumes that the planet is surrounded by an atmosphere extending for a depth of 3,000 miles beneath the visible cloud-bearing strata forming the apparent surface of the planet. It has been also shown that the greatest depth of atmosphere which the planet Jupiter can thus possess does not exceed one-fifteenth to one-twentieth of this depth. Consequently the phenomenon observed by Mr. Todd and Mr. Ringwood cannot have had its origin in the way supposed.

It may be raised as an objection, that the method which has been employed in dealing with this question is unsatisfactory, for theoretical considerations in connection with the physical condition of the atmosphere of a planet appear strange to most astronomers. To show that the phenomenon could not have arisen from the atmosphere of Jupiter in the manner thought, because this supposed that the atmosphere had a very great depth, whilst it could theoretically be shown not to have a tithe of this great depth, appears to be a very sound argument. Some may object, with perfect truth, that although it has been shown that it is impossible for the phenomenon to have arisen from an atmosphere 3,000 miles deep, because such a depth cannot exist, the phenomenon may have arisen from the effect of the atmosphere of Jupiter after all. For they may urge only one way has been considered in which the phenomenon could have arisen, and there may be a hundred. There are others who may consider one grain of observation worth a bushel of theory. These overlooking the fact that the interpretation to be put on the observations is pure conjecture, whilst the theoretical results can be put on as incontestable footing as the numerous facts derived from theory, which they are content to accept unquestioned, may refuse to accept the proof that the atmosphere of Jupiter cannot possess a greater depth than 200 miles, on the ground that observations exist which they think may indicate a depth twenty times as great.

To meet these objections a further step will be taken, and it will be shown, first, that the phenomenon observed by Mr. Todd and Mr. Ringwood could not have arisen from the atmosphere of Jupiter: secondly, that an atmosphere to Jupiter would have produced very different phenomena entirely; and thirdly, that a very simple explanation of the phenomenon can be obtained without any reference to the atmosphere at all.

(To be continued.)

ON THE LUNAR VALLEY J. J. CASSINI OF SCHRÖTER.

By Dr. A. Schafarik.

Mr. W. R. Birt has, on different occasions, pointed out (last time, if I am right, in *Astronomical Register*, No. 153, p. 224, Sept., 1875) the totally different manner in which this interesting lunar region is represented by Schröter and by Beer-Mädler. This difference struck me also five years ago, when I saw J. J. Cassini for the first time, and found it quite in accordance with Schröter's drawing. The high authority of Beer and Mädler, who could not find the slightest resemblance between nature and Schröter's drawing, induced me carefully to compare the latter with the *Mappa Selenographica*, with all photograms available to me, and with my own impressions, and it is the result of this comparison which I now take the liberty to communicate. The following lines are taken from a memoir on physical selenography, presented to the Royal Society of Bohemia in January, 1874, but not yet published.

The name of John James Cassini was given by Schröter to "a remarkably large valley, bordered by cliffs, and situated in the region of Riccioli's Meton and Euctemon." Schröter describing it in § 779 of his *Selenotopographical Fragments* (Vol. II.), finds it resembling the valleys of the Harzgebirge, and depicts it on Tab. 61, fig. 2, as he saw it in 1788, Nov. 21, at 6h. 45m.—8h. in the morning, 16 hours after last quadrature. Beer-Mädler say on it, p. 211, (after having discussed the doubtful identity of some of Riccioli's objects) "In like manner we cannot identify in this region some names of Schröter's, for instance Lexell and James Cassini. To admit the correctness of his drawings it would be necessary to suppose that the lunar globe has suffered most enormous changes in the course of 30 years."

This sentence I did know before I had seen J. J. Cassini in the telescope, and, to say the truth, on a superficial comparison of Schröter's Tab. 61 with the *Mappa Selenographica*, there seems to be no trace of resemblance. I was the more surprised, when on August 14, 1872, at 8h. in the evening, (☽'s δ —24° terminator in λ —41°), notwithstanding the high angle of illumination, I found the valley quite a likeness of Schröter's drawing. A note taken at the time says, "Air bad; 120 almost of no use (silver on glass reflector, 4 inches aperture); Schröter's valley J. J. Cassini wonderful, quite a terrestrial aspect; in the broad part $\kappa\lambda f$ beautiful shadows of the mountain masses $\Delta\gamma$; the deep circular basin $\gamma\beta\eta Z$ distinct; clouds intervened at 9h." Since that time I have seen the valley on nine different evenings, under very varying angles and varieties of illumination, but never failed when the phases were similar to that of 1788, Nov. 21, to be struck by the extraordinary fidelity with which Schröter's coarse drawing renders the general character of the objects, a fidelity which reminded me of a dictum of my venerable master, Wöhler, who told me in 1858, when I was working on Vanadium compounds in his laboratory at Gottingen, "Such was the extraordinary care and fidelity of Berzelius, that if you repeat his experiments exactly under the conditions given by him, you may be sure also to fall back on his results."

My observing notes being very short, and no sketches being taken by me of this particular object, I can only make some general remarks on J. J. Cassini. Under high southern librations (*i. e.*, the south pole brought nearer to the apparent centre) the valley is so much foreshortened that detail is lost (so 1873, May 6, and December 30: 1876, October 11); northern libration on the contrary, makes the scenery much more conspicuous and full of detail. So 1874, June 8, when the shadows in the terminal basin B Z were already gone, the defile γ Aα B was quite black, whilst the eastern border or coast B*b* threw on the flat bottom of the large bay *e f*, a splendid black semicircular shadow, crowned at both ends by shadow cones, and the double mouth of the fiord into the Mare Frigoris was well marked. The basin B Z is surrounded by a chain of detached mountains, which form a ring of bright points when in the dark hemisphere and near the terminator; the high peak ξ throws a long double spire of shadow when conveniently illuminated, and the bottom of the basin is traversed from N. to S. by four parallel ridges, which are well given on *Mappa Selenographica*, and with some attention can be made out even when much foreshortened by libration.

By careful comparison I found immediately that very nearly all objects figured by Schröter are represented in the *Mappa Selenographica*; nay, you can trace the contours of J. J. Cassini in the latter without much searching, if you have seen it once in nature; but certainly in this particular case Schröter's clumsy drawing gives a more adequate idea of the general aspect and peculiar character of that spot than the *Mappa Selenographica*, which is drawn in the conventional manner of Lehmann. The identification of the objects common to both maps is as follows:

Fontenelle, *Schröter* =	Fontenelle *Beer-Mädler*
B	Fontenelle Δ
v	δ
f	hooked mountain within the quadrangular rampart near Fontenelle, between ε ζ

cd	fragments of the southern rampart ε
e	hill, south of Fontenelle A, the western of two
ba	ridge towards Plato δ
h	Fontenelle B
m	6
ω	2
s	Condamine B
qr	two craters between Condamine B and Fontenelle C, *r* on a bright streak, *q* south of it
u	crater in the grey plain, λ —30° β + 64°
p	Fontenelle C
ζ	craterlet and mountain group south of Fontenelle C
λ	Fontenelle γ
Z	Philolaus *e*
ι	Anaxagoras γ
H	the terminal basin, a mountain ring Anaxagoras γ β η Z
Epigenes *Schröter*	Philolaus *Beer-Mädler*, and ring of mountains, Philolaus γ CB
α	mountainous tract between Philolaus B 6
ε	Philolaus *d* E
β	craterlet Philolaus B
γ	craterlet in the grey plain south of Anaximenes, β +67° λ —35°
Lexell, *Schröter*	Anaximenes, *Beer-Mädler*
δ	a crater *wanting* in MS.
Philolaus *Schröter*	Anaximenes A 6 *Beer-Mädler*
Anaxagoras *Schröter*	Anaximenes cd *Beer-Mädler*
	(Anaxagoras, *Beer-Mädler*, was on the unilluminated hemisphere at the time of Schröter's drawing.)

With regard to Schröter's Lexell, Beer-Mädler, who have transferred this name to a ring-mountain in the III. quadrant near Tycho, remark (p 301), "In Schröter's fragments the name Lexell is found in a region near the north pole, which we are at a loss to understand from his drawing, so that we found no other means than to give this name to another lunar object in a far distant part of the moon, lest we should strike out the name of so meritorious an astronomer, or give rise to new confusion by identifying Schröter's Lexell quite at a guess."

From what I have shown, there was, I think, no necessity to make such assumptions, and this is not the only instance in which the two learned selenographers, whose merit is so immense that it cannot be lessened by remarks on nomenclatural defects, have not respected the names given by Schröter in the manner sanctioned long ago in the descriptive branches of natural science. So strict are the nomenclatural rules of the latter, that a name given in violation of them has no claim to be respected, and every subsequent worker has a right to substitute for it another name, chosen in strict accordance with the rules. That the latter procedure should be avoided in selenography as much as possible no one can doubt: who knows the amount of trouble brought on unfortunate botanists and zoologists by endless lists of synonyms?

The Rev. T. W. Webb has pointed out that Beer-Mädler have exchanged Schröter's names of Dörfel and Leibnitz, obviously by oversight (*Selenotopographical Fragments*, I., 132 and 139), and proposes to restore their original meaning; this would necessitate our writing Dörfel *Schröter*, and Dörfel *Beer-Mädler*, Leibnitz *Schröter*, and Leibnitz *Beer-Mädler*, as in botany and zoology. But botanists and zoologists have given us an excellent example in deciding that the systematical names of Linné, who has given the first truly scientific conspectus of the *whole* of organic nature, should be regarded as *fundamental* ones, and that names given by his predecessors, be they ever so great, like immortal Tournefort, should have no right of priority; and so it would not be unjust to state by general convention that the names given by the authors of the first *exact* special map of the *whole* lunar hemisphere should not be changed on account of some few objects having been differently named by previous workers, be they ever so merited as the venerable old fragmentist of Lilienthal.

In some of the lunar photograms which I possess J. J. Cassini is very well drawn, so in Rutherfurd, I. and III. (*Atlas to Proctor's Moon*, large edition), in the fine photogram by Mr. Brothers, 1865, Dec. 27 (frontispiece of Proctor's book), also in Rutherfurd, IV. (taken 1865, March 6, and published in 1873 in a fine Woodbury type, as a frontispiece to Prof. H. Vogel's book on light).

The photograms give sanction to Schröter's figure; but they show opposite to the eastern semicircular bay a similar one on the western shore, by which the section of the valley contained between β $+65°$ and $70°$ is transformed into a wide flat oval basin, evidently an imperfectly developed ring-mountain, or walled plain, united by the narrow valley Ba Δ with another similar but better developed form, the terminal basin B Z, so that the whole falls under the general type of lunar formations, only more disguised in that particular case. The latter remark has already been made by Mr. Birmingham, if I am right; but, being not at home and without books, I am not sure of it.

On Rutherfurd I. (terminator λ —30°) the narrow fiord *a*—*e* and the terminal basin are dark, only a faint trace of a light streak sweeps over the bottom of the latter towards the eastern wall, indicated by four bright peaks; the light falls through a gap in the western wall near B. Anaxagoras γ throws a shadow of some 22″ length, and receives itself the shadow of η. The eastern coast is broken at *b f*; its southern course forms a gracefully swung, bright, sharp curve, below which two faint ridges are to be traced on the widened, flat bottom. The western-border throws a strong shadow, that of γ occupying two-thirds of the width of the valley towards λ, that of Fontenelle B also; the slope between A γ is very bright. The valley debouches into the Mare by two pretty large grey arms, separated by the mountain mass 26. The delicate isolated mountains bordering at some distance the coast κ λ and B 6 are very distinct.

On Rutherfurd III. (terminator +15°, moon decreasing, libration more favourable than in I.) the valley is quite open, and very much resembles Schröter's drawing (with the exception of the bay in the western border, just alluded to), notwithstanding the higher angle of illumination. The shadow Anaxagoras γ is less pointed than in I.; more broad than long; the terminal basin has little shadow at Z, its western border is bright, and hardly to be distinguished from the mountain highlands in its base; the most eastern of Beer-Mädler's four ridges throws a faint shadow; the same is the case with Anaxagoras B, the mountains between B and γ, and with Δ on the bright western border; Schröter's bright crater Z (Philolaus *e*) is not visible; at β —70° the coast forms a promontory, and from that to γ a convexity, on the widened bottom only crater *e* is visible. Two narrow, and slightly divergent, bright streaks pass from Anaxagoras over γ λ, including the distinct double crater C, and form, apparently, a narrow mouth of the valley into the Mare. They reach nearly to Bouguer, and are brighter and more rectilinear in nature than on the M.S.

On Rutherfurd IV. (terminator λ —22°) J. J. Cassini is on the terminator and shows little detail. The terminal circus is pointed out by five bright and some faint peaks (near B and Z), on its dark floor the highest of the four meridional ridges shines forth; the eastern side of the valley is traced by a chain of numerous bright peaks, particularly at *a* B and in the hemicycle *b* γ δ λ, ending at the latter. All the rest is dark, except two bright points between Fontenelle A and Philolaus *b* γ, which may be the craters *e f*, or *f* and mountain γ. This astonishingly rich and sharp photogram is taken almost at extreme north-western libration, and with a terminator a little more advanced it would be highly valuable to the topography of J. J. Cassini.

Mr. W. R. Birt has already pointed out in his first note on J. J. Cassini (*Astronomical Register*, No. 131, Nov., 1873) that the confusion Beer and Mädler found in this region is by no means to be put on Schröter's account, and the Rev. T. W. Webb has admitted the Vallis Cassini in his index map of the moon (*Celestial Objects*, 3rd edition, 1873), though he has retained Schröter's incorrect rectilinear figure of the western side.

Prague: 1877, July 21.

SATELLITES OF MARS.

Letter to the Hon. R. W. Thompson, Secretary of the Navy, announcing the discovery of satellites of Mars.

U. S. Naval Observatory, Washington,
August 21, 1787.

Sir,—The outer satellite of Mars was first observed by Professor Asaph Hall, U. S. N., on the night of the 11th August, 1877. Cloudy weather prevented the certain recognition of its true character at that time. On August 16th it was again observed, and its motion was established by observation extending through an interval of two hours, during which the planet moved over thirty seconds of arc. The inner satellite was first observed on the night of August 17th, and was also discovered by Professor Hall. On Saturday, August 18th, the discoveries were telegraphed to Alvan Clark and Sons, Cambridgeport, Mass., in order that if the weather should be cloudy at Washington, they might confirm the existence of the satellites with the 26-inch telescope of Mr. McCormick, which is in their hands. This discovery was confirmed by Professor Pickering and his assistants at Cambridge, Mass., and by the Messrs. Clark, at Cambridgeport.

On August 19th the discovery was communicated to the Smithsonian Institution, by which it was announced to the American and European observatories in the following dispatch: *Two satellites of Mars discovered by Hall, at Washington. First, elongation west, August* 18, *eleven hours, Washington time. Distance eighty seconds. Period thirty hours. Distance of second, fifty seconds.* It will be seen hereafter that the statement of fifty seconds as the distance of the inner one was erroneous. [Then follows a series of observations on the satellites.] From these observations Professor Newcomb has derived the following approximate circular elements of the orbits. The probable errors assigned are only very rough estimates.

THE OUTER SATELLITE.

Major semi-axis of apparent orbit seen at distance	[9·5930]	82″ 5 ± 0″·5
Minor " " " " "	[9·5930]	27″·7 ± 2″
Major semi-axis of orbit seen at distance unity ...	...	32″·3
Position angles of apsides of apparent orbit ...		70°, 250° ±2°
Passage through the west apais ($p = 250°$),		Aug. 19th, 16·6h., W. m. t.
Period of revolution		30h. 14m. ± 2m.
Hourly motion in Areocentric longitude ...	...	11°·907
Inclination of the orbit to the ecliptic	...	25°·4 ±2°
Longitude of ascending node	...	82°·8 ±3°
Position of pole of orbit in celestial sphere ...	...	Long. 352° 8 Lat. +64° 6

These elements give for the mass of Mars $\frac{1}{3{,}090{,}000}$ R.A. 316·1
Decl. +53·8

THE INNER SATELLITE.

Major semi-axis of apparent orbit at distance [9·5930] ... 33·0 ±1″
Period of revolution ... ... 7h. 38·5m. ±0·5m.
Hourly motion in Areocentric longitude ... ... 47°·11
Passage through the eastern apsis ($p = 70°$) Aug. 20, 13·0h. W. m. t.

Very respectfully your obedient servant,
JOHN RODGERS.
Rear-Admiral and Superintendent.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

THE NEW STAR IN CYGNUS.

Sir,—Some of your readers may be interested in hearing that the spectrum of this star is now reduced almost to a single bright line. This is the line that was described by Cornu in December last as being situated at wave length 500 (my observations make it rather more), and it was then far from being the brightest line in the spectrum, but it has been the brightest ever since Jan. 8. The star continues to fade with extreme slowness.

Sunderland: August, 1877.
Yours truly,
T. W. BACKHOUSE.

THE ROYAL ASTRONOMICAL SOCIETY.

Sir,—My good friend Mr. Dunkin has altogether misunderstood me. I wrote in all friendliness to point out that he was making too much of a matter which really is quite without importance. He manifestly supposes it is one in which I take a great deal of interest; whereas I did not even attend the meeting at which the events happened which he speaks of as "deplorable," and I have not to this day read the proposed alterations of the Bye-laws once carefully through. Yet I supposed the matter would be disposed of at the June meeting, and I also had a special reason for desiring to attend that meeting, having been asked by my friend Capt. Noble to record my vote for Mr. Denning. But I could not even for that purpose waste two hours of my time over one of the fighting meetings. As to being desirous that the general body of the Fellows should adopt my views, I have no views for them to adopt, and have therefore, of course, expressed to none of the Fellows, publicly or privately, an opinion about any of the proposed changes. The only views I have, relate to Mr. Dunkin's altogether startling proposition in the July number of the *Register*, and what I have said on that point was said in the interests of peace and good fellowship.

He now addresses to me certain explanations about the Astronomical Society's Club. I am obliged to him; but I have never said anything about the Club, care nothing about it, and know nothing about it, except what he is good enough to tell me: as to this, if he had assured me that the members of the Club, as a rule, prefer sole to salmon, and take sherry with their soup, the information would have been quite as much to the purpose, so far as I am concerned. Nor have I ever spoken of a club-party or clique. How then *I* can have reduced the club-party by excepting Sir G. Airy, Prof. Adams, and Mr. Lassell, passes my comprehension. I simply selected those three names as illustrations of the amazing nature of the proposition deliberately advanced by Mr. Dunkin, that they, Lord Lindsay, Mr. Knott, and several others, should secede from the Council.

Here I must call my friend's attention to the use of an expression which, if meant as it certainly would be understood, is not fair towards me, or towards Capt. Noble. He says, " we *now* delight to honour," or " *now* so delight to honour," Sir George Airy and Prof. Adams. Now, so far as Capt. Noble is concerned, I may remark (merely in passing) that he has been for many years one of my most valued friends, and I have had many opportunities of exchanging ideas with him: invariably his private expression of opinions respecting both Sir George Airy and Professor Adams have corresponded with what he has as invariably expressed publicly; in other words he has constantly expressed the highest esteem and respect for both. I may add that, at a time when I thought I had not been justly treated by Sir G. Airy, Capt Noble urged upon me most strongly the propriety of attending a meeting of the Society for the special purpose of explaining a remark which had been misunderstood (perhaps the fault was mine) as reflecting upon the Astronomer-Royal. Mr. Dunkin may be assured that Capt. Noble's representations were very strong or I should most certainly not have yielded to them—and he will understand that they were based not only on strong views, but on what I regarded at the time as exaggerated views of what was due to the Astronomer-Royal. But so far as *I* am concerned, I must confess I hear with great surprise that I *now* specially delight to honour the justly honoured names of the Astronomer-Royal and the discoverer of Neptune. Can Mr. Dunkin have conceived so meanly of me (and yet continue to address me as a friend) as to imagine that because I maintained, when occasion required, a different opinion from that advocated by Sir G. Airy, and because in the progress of the controversy I indicated definitely where, in my judgment, his arguments were incorrect, while he in turn pointed out what he regarded as flaws in my reasoning, I gave up *then*, and only resume (ungraciously) *now* the opinions which I had before expressed both publicly and privately of his distinguished services to science? Or does he imagine that aught which has happened since has created—I will not say rancour or ill-feeling against Sir George Airy, for the idea of either is too preposterous, but—even indifference to the noble results which science owes to him? All I can say is that, if he does, he misjudges me grievously. I should utterly despise myself, I should consider I had become mentally or morally imbecile, if it were so. With regard to Prof. Adams, I have not had the least difference, even of opinion, nor have I ever, either publicly or privately, expressed any feeling respecting him but that which Mr. Dunkin strangely describes as if it were of recent growth. I may remark in passing that of my two chief works in the last five years, I dedicated one to Sir George Airy in 1872 (in the full heat of the transit controversy) and the other to Prof. Adams in 1874, a few months only after my retirement from the Council of the Astronomical Society, and when it was perfectly well known to me that he had supported, with Messrs. Whitbread, Knott, and Main, Sir G. Airy's objection to the Council's selection of my name for the medal. Can *this*, by the way, be Mr. Dunkin's reason for imagining, as he seems to imagine, that my respect for Prof. Adams's eminent services to science had been modified? Truly, if so, I cannot thank him for the compliment.

But Mr. Dunkin goes on to make some statements respecting Sir G. Airy and Prof. Adams, which, as the only statements in the slightest degree bearing on my letter, must I suppose be intended as corrections of misrepresentations which he charges me with making. I shall be surprised to hear, he thinks, that both these gentlemen were strong supporters of the proposed amended Bye-laws, that to the former the Council were indebted for many valuable suggestions, and in fine that Sir G. Airy was one of the originators of the proposed alterations. I am not in the least surprised to hear any one of these things, but I *am* utterly surprised that Mr. Dunkin should advance them in correction of anything said by myself. I shall not be surprised to learn that the Society, as a whole, strongly supports the amended Bye-laws (not necessarily without modification in detail), that it recognises the value of Sir G. Airy's suggestion, and that it justifies by its approval the action of Sir G. Airy in proposing alterations of the Bye-laws. What the special meeting objected to, as I understand, was the want of opportunity to examine and discuss the suggested changes, and I should certainly be very much surprised indeed to learn that either Sir G. Airy or Prof. Adams considered that the Society ought to have accepted the changes without proper examination. As the matter could not possibly be dealt with unless amendments could be proposed, and as our present laws make no provision for amendments in such cases, adjournment was the only available course. If Mr. Dunkin did not misunderstand Prof. Adams, and the latter really expressed the opinion that the adjournment to February 1878 implied want of confidence in Sir G. Airy, Prof. Adams, and the others named by Mr. Dunkin, then I think (and am glad to think) that Prof. Adams was altogether mistaken. On one point I am very certain. The endowment party have most persistently endeavoured to spread abroad the notion *that Sir G. Airy favours their schemes, but he does not in the slightest degree.*

Mr. Dunkin's proposing to bow to the decision of the Fellows next February, "whether in favour of the management of the Society by the majority in the present Council, or by a Council composed entirely of fresh blood," would be very much to the purpose if there were any chance of such an issue being submitted to the meeting. But there is not and cannot be.

What he means by "working astronomers," let me remark in passing, is not altogether clear. Does he by any possibility mean professional astronomers, or those occupying definite posts in public observatories? Both the words "working" and "astronomer" have a rather wide signification. Narrow-minded persons may limit astronomy to a small class, according to fancy or prejudice. One will say the professional observer (the more meridional the better) is alone to be called an astronomer; others may take the other extreme view, and say that the work at public observatories bears no closer relation to real astronomy than land-surveying or civil-engineering bears to geology. But all this would be petty in the extreme. Our motto is a wide one, *quicquid nitet notandum*, and we are not going to tell one man he is not an astronomer because he never took a transit (which would exclude Newton), or another because he never observed a total eclipse (which would exclude Sir J. Herschel); one class because they do not rejoice in mathematical astronomy, or another because they are weak in the physics of astronomy, or its history, or in the matter of double, or red, or variable stars, or the moon, or comets, and so forth—*usque ad nauseam.*

I am certainly *not* concerned by Mr. Dunkin's reference to anonymous writing in the way he suggests. What I objected to was his classing a writer of anonymous letters, who, though his name is not published, is known to and has the sanction of an editor, with the really anonymous letter-writer. One of the latter sort, at least if he attacks others (for it is possible that a really anonymous letter may be written for a good end, however unwisely in reality), is necessarily a dastardly and utterly contemptible scoundrel.* But no stigma attaches to anonymous letter-writing of the former kind. (I speak quite disinterestedly, for I do not remember ever writing an anonymous letter to any journal in my life, though I have written not a few anonymous articles.) I really think that Mr. Dunkin's wrath about the letters he actually refers to is a good deal exaggerated. So far as he is concerned, all that was said was that beyond doubt he would be able to contradict the rumour that he now votes for schemes (which he certainly opposed formerly) because he has received kindness at the hands of a supporter of those schemes. If the anonymous letter-writer had said the reverse—viz., that Mr. Dunkin could *not* contradict the rumour, he would have had reason to be angry. I think the worst said by the writer in question about any one was not very severe. But I must confess I have long since ceased to take interest in the endowment matter. I took part in the controversy once, but that was when I had more leisure. At present I fear that if ten thousand a year each were given to all who want money from government, and we heard no more about them, I should in my selfishness pay my share of whatever extra tax was wanted, and thank God for peace and quiet at the price. My valued friend Capt. Noble (possibly as one of the Commissioners of Inland Revenue) considers, I know, that close watchfulness in the matter is a positive duty. It may be so, but the duty is one in the discharge of which I for one have been and am altogether negligent. *Ars longa, vita brevis.*

RICHARD A. PROCTOR.

NEW COMET.—M. LE VERRIER.

Sir,—A new comet was discovered at Marseilles by M. Coggia, on Sept 14, in R.A. 8h. 32m. 3s.; Decl. +48° 14′. Its R.A. was decreasing daily at the rate of 45s., and its Declination at the rate of 18′. It is described as being very faint.

I may take this opportunity of stating that a few days ago, when in Paris, I called to see M. Le Verrier, and learnt with extreme regret that he was not only dangerously ill, but that an early fatal termination to his illness was expected. Such an event would awake the liveliest regrets all over the world.

Observatory, Eastbourne: Your obedient servant,
Sept. 25, 1877. G. F. CHAMBERS.

[We are sorry to know that our friend's fears were too well grounded.]

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* Not but that there are those who will lie and quibble as contemptibly without a mask as with one. Let me take this opportunity of noting the utter untruth of the statement which has been made in a contemporary periodical, that my friend Capt. Noble has a share in some magazine, and that his opposition to the endowment scheme has, therefore, a "business" interpretation. He neither has nor ever has had a share in any publication whatever. If the statement had been made about myself it would have been untrue, but there might have been some explanation of the mistake. Made respecting Capt. Noble, the statement is not only untrue, it is so utterly inconsistent with all probabilities that it cannot have had its origin in misapprehension, but must be regarded as a wilful and deliberate falsehood. It is as discreditable to the editor who has permitted its appearance, knowing it to be untrue, as to the person who has uttered it,—who, unfortunately, is beneath Capt. Noble's contempt, and may thus escape the castigation he most justly merits.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN OCTOBER, 1877.

By W. R. Birt, F.R.A.S., F.M.S.

Fontenelle.—We solicit attention this month to this formation and its surroundings. It is described by Neison as a fine ring plain 22·73 miles in diameter, its east wall rises 6,069 feet above the interior, which contains a small central peak. It is situated in area II. I. ξ of the moon as it has been divided for the registration of lunar objects, and is No. 1 of that area. On referring to Schröter's drawing T. LXI., fig. 2 of his Fragments, we find several objects surrounding Fontenelle which it is desirable should be re-observed, we shall accordingly specify them nearly in the order of their proximity to the larger crater.

II. I. ξ 9.—A mountain on the south-east of Fontenelle, marked *g* by Schröter, who depicts it with most of the objects hereafter to be mentioned, as he saw them on the morning of Nov. 21, 1788, the shadow is directed towards the west; it is that of a cone terminating in a point. B. and M. give in its place a mountain on their map, but undesignated. The length of its shadow is little more than half that of Fontenelle, which is a little less than half the length of the floor of Fontenelle.

II. I. ξ 2.—A somewhat extensive mountain mass on the north-east of Fontenelle, marked *h* by Schröter, and B by B. and M. Neison places it in 62° 48′ N. lat., and 19° 44′ E. long.; he also says it is steep and high. From this mountain to B. and M.'s γ is a range of mountains apparently a portion of the south part of the west border of the large valley J. J. Cassini. Schröter does not show this as mountainous, the mountainous character of the west border of J. J. Cassini is indicated on T. LXI., fig. 2, by *x*, *x*, *x*.

II. I. ξ 8.—A mountain on the west of II. I. ξ 2, marked *i* by Schröter, it is shown by B. and M.

II. I. ξ 4.—A ridge extending from Fontenelle to the crater Schröter B; (B. and M. A.) This ridge starts from the north-west rim of Fontenelle, it terminates at Schröter's crater B, and is parallel with part of the west border of J. J. Cassini; it is marked *k* by Schröter and is well represented by B. and M.

II. I. ξ 6.—B. and M. discovered a quadrangular enclosure west of and nearly adjoining Fontenelle. Neison gives a much clearer figure of it than B. and M. The sides of this enclosure face respectively the S.E., the S.W., the N.W., and the N.E.; the eastern angle projects into this area. Schröter does not mention it.

II. I. ξ 7.—The southern part of the north-east wall of II. I. ξ 6.

II. I. ξ 5.—A mountain peak on II. I. ξ 7, marked *v* by Schröter and δ by B. and M.

II. I. ξ 3.—The south part of a broad plateau north of Fontenelle. Schröter did not observe this plateau, at least he does not appear to have specially noticed it. This plateau is marked γ by B. and M.; its general elevation is about 2,660 feet, it extends into the lunar Zone XXVII. described in the *Astronomical Register*, No. 153, p. 224, where its salient angle II. I. τ 6, rises to the height of 5,333 feet. The eastern side of the plateau is most probably the curved position of the ridge forming the south part of the west border of J. J. Cassini. It is given by Neison, but we should like to receive some good drawings of it marking out its boundaries.

The above are all the objects as yet registered in area II. I. ξ 60° to 65° N. lat., 15° to 20° E. long.; they are more or less south of the great valley J J. Cassini, which being situated so far north its appearance is greatly affected by libration; it is best seen when the moon has south latitude.—*Report British Association*, 1866, p. 243, note. The careful study of Schröter's drawings T. LXI., fig. 2, will greatly assist in obtaining a correct knowledge of the valley. The above is intended as a first instalment towards this work.

THE PLANETS FOR OCTOBER.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	11 54 34	S. 0 12½	9″·3	23 9·5
	9th	12 0 9	N. 1 29	7″·4	22 43·6
	17th	12 35 37	S. 1 37	5″·9	22 47·5
	25th	13 22 24	S. 6 49½	5″·2	23 2·7
Venus ...	1st	14 54 24	S. 17 42	14″·1	2 12·7
	9th	15 32 46	S. 20 38½	14″·7	2 19·6
	17th	16 12 15	S. 23 2	15″·5	2 27·5
	25th	16 52 37	S. 24 48	16″·5	2 36·3
Mars ...	1st	22 47 41	S. 12 29	25″·4	10 4·7
	9th	22 46 49	S. 11 52½	23″·6	9 32·4
	17th	22 49 9	S. 10 58	21″·6	9 3·3
	25th	22 54 21	S. 9 49½	19″·8	8 37·0
Jupiter ...	1st	17 44 26	S. 23 22	34″·5	5 2·3
	9th	17 48 57	S 23 24	33″·9	4 35·4
	17th	17 54 4	S. 23 26	33″·3	4 9·0
Saturn ...	1st	23 9 24	S. 7 52½	17″·2	10 26·4
	9th	23 7 31	S. 8 3½	17″·0	9 53·1
	17th	23 5 54	S. 8 13	17″·0	9 20·0
	25th	23 4 36	S. 8 20	16″·8	8 47·2
Neptune ...	4th	2 20 19	N. 12 4	...	13 25·0
	20th	2 18 42	N. 11 55	...	12 20·5

Mercury may be observed in the morning for rather more than an hour before sunrise at the beginning of the month, the interval increasing to an hour and three-quarters by the 13th, and then decreasing to an hour.

Venus sets about an hour after sunset on the 1st, the interval slightly increasing.

Mars is visible till three hours after midnight on the 1st, the interval decreasing.

Jupiter sets about three hours and a quarter after sunset on the 1st, the interval decreasing.

Saturn sets nearly four hours after midnight at the beginning of the month, the interval decreasing.

NEW MINOR PLANETS (174), (175).

From the *Astronomische Nachrichten.*

New minor planet, discovered by Borelly, Marseilles.

August 11th, 8h. 35m. 0s. R.A. 21h. 19m. 50s. P.D. 105° 59′.

Daily motion in R.A. 60 seconds. In P.D. 8 minutes. Magnitude 10th.

The same discovered by Watson, Ann-Arbor, August 8th.

August 16th, R.A. 21h. 14m. Dec. 15° 47′. Movement 68 seconds in Dec. 2 minutes. 10th mag.

The following telegram has been received at the Berlin Observatory from Washington, touching the discovery of the planet (175).

Watson's Planet, Ann Arbor.
1877. September 3. 23h. 10m. +0° 45′
Daily motion, —55s. —1′ Mag. 11.

Owing to unfavourable weather I have obtained only the following approximate observation :

September 4-5. Berlin 23h. 8m. 25s. +0° 42′·1

Berlin : 1877, September 5. (Signed) V. KNORRE.

DISCOVERY OF A COMET.

Telegraphic despatch from the Imperial Academy of Sciences at Vienna to the Editor.

Comet Coggia, 13 September, 16h. 40m. M.T., Marseilles.
$\alpha = 128° 11'$, $P = 41° 30'$. Motion $-11'$, $+17'$; faint.
Vienna : September 15, 1877.

—From *Astronomische Nachrichten*, No. 2155.

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF MARS.

Greenwich Noon. 1877.	Areographical longitude of the centre of ♂'s disc.	‖	Areographical latitude of the centre of ♂'s disc.	Angle of position of southern pole.	Diameter of disc.	Amount and position of greatest defect of illumination.	
	°	°	°	°	″	″	°
Oct. 1	303·9	350·9	—23·80	163·47	21·47	0·72	59·9
2	294·8	351·0	23·87	163·51	21·28	·76	60·4
3	285·8	350·9	23·95	163·54	21·09	·80	60·8
4	276·7	·9	24·03	163·56	20·89	·84	61·2
5	267·6	·9	24·11	163·58	20·69	·87	61·5
6	258·5	·8	24·19	163·59	20·49	·91	61·7
7	249·3	350·9	—24·27	163·59	20·29	0·94	62·2
8	240·2	·8	24·35	163·58	20·09	0·98	62·5
9	231·0	·8	24·44	163·56	19·89	1·01	62·8
10	221·8	·9	24·52	163·53	19·69	1·04	63·0
11	212·7	·8	24·61	163·49	19·49	1·07	63·3
12	203·5	·7	24·69	163·44	19·29	1·10	63·5
13	194·2	·8	24·78	163·39	19·09	1·13	63·7
14	185·0	350·8	—24·87	163·33	18·89	1·16	63·9
15	175·8	·7	24·96	163·26	18·69	1·18	64·1
16	166·5	·7	25·05	163·18	18·49	1·21	64·3
17	157·2	·7	25·14	163·10	18·29	1·23	64·4
18	147·9	·7	25·23	163·00	18·09	1·25	64·6
19	138·6	·7	25·32	162·90	17·90	1·27	64·7
20	129·3	·7	25·41	162·79	17·71	1·29	64·9
21	120·0	350·7	—25·50	162·67	17·52	1·31	65·0
22	110·7	·6	25·59	162·54	17·33	1·33	65·1
23	101·3	·6	25·68	162·41	17·14	1·35	65·2
24	91·9	·7	25·77	162·27	16·95	1·37	65·3
25	82·6	·6	25·85	162·13	16·77	1·38	65·4
26	73·2	·6	25·94	161·98	16·58	1·39	65·5
27	63·8	·6	26·03	161·82	16·40	1·40	65·6
28	54·4	350·6	—26·12	161·66	16·22	1·41	65·7
29	45·0	·5	26·20	161·49	16·04	1·42	65·7
30	35·5	·6	26·29	161·31	15·86	1·43	65·8
31	26·1	·5	26·37	161·13	15·69	1·44	65·8
Nov. 1	16·6		26·45	160·95	15·52	1·45	65·9

A. M.

ASTRONOMICAL OCCURRENCES FOR OCTOBER, 1877.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Mars. —
Mon	1	14 24 17 54 18 21 18 11	Near approach of η Cancri (6) Occultation of 39 Cancri (6) Reappearance of ditto Near approach of 40 Cancri (6)	3rd Oc. D.	8 41	10 4·7
Tues	2		Sidereal Time at Mean Noon, 12h. 45m. 14·07s.			10 0·5
Wed	3	14 45 14 56	Near approach of 44 Leonis (6) Near approach of B.A.C. 3562 (6½)			9 56·4
Thur	4		Sun's Meridian Passage 11m. 22·14s. before Mean Noon			9 52·3
Fri	5	10	Conjunction of Moon and Mercury 3° 1′ N.	3rd Sh. E. 2nd Tr. I.	6 46 7 17	9 48·2
Sat	6	9 58	● New Moon			9 44·2
Sun	7			1st Oc. D. 2nd Ec. R.	6 9 7 5 35	9 40·2
Mon	8			1st Tr. E 1st Sh. E.	5 38 6 52	9 36·3
Tues	9	11	Conjunction of Moon and Venus 3° 47′ N.			9 32·4
Wed	10					9 28·6
Thur	11	19	Conjunction of Moon and Jupiter 4° 35′ N.			9 24·8
Fri	12	4 43 5 47	Occultation of B.A.C. 6220 (6½) Reappearance of ditto	3rd Tr. E. 3rd Sh. I.	5 51 7 44	9 21·1
Sat	13	15 42	☽ Moon's First Quarter			9 17·5
Sun	14			1st Oc. D.	8 7	9 13·9
Mon	15		Illuminated portion of disc of Venus=0·726 Illuminated portion of disc of Mars=0·937	1st Tr. I. 1st Sh. I. 1st Tr. E.	5 20 6 31 7 37	9 10·3
Tues	16		Sidereal Time at Mean Noon 13h. 40m. 25·82s.	1st Ec. R.	6 1 35	9 6·8

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Mars.
Wed	17	22	Conjunction of Moon and Mars 4° 28′ S. Saturn's Ring: Major axis=42″·37 Minor axis=2″·68			9 3·3
Thur	18	7	Conjunction of Moon and Saturn, 3° 54′ S.	4th Ec. R.	6 41 36	8 59·9
Fri	19		Sun's Meridian Passage 15m. 1·34s. before Mean Noon	3rd Tr. I.	7 9	8 56·5
Sat	20					8 53·1
Sun	21	19 30 15 41 16 31	○ Full Moon Occultation of 104 Piscium (6½) Reappearance of ditto	2nd Oc. D.	7 14	8 49·8
Mon	22			1st Tr. I.	7 19	8 46·6
Tues	23	4 51	Occultation reappearance of ε Arietis (4½)	2nd Tr. E. 2nd Sh. E.	4 51 7 8	8 43·8
Wed	24			1st Sh. E.	5 12	8 40·2
Thur	25					8 37·0
Fri	26			4th Tr. E.	5 49	8 33·9
Sat	27	16 41 17 56	Occultation of A Geminorum (5½) Reappearance of ditto			8 30·9
Sun	28	9 30 9 40 23	Occultation of μ^1 Arietis (6) Reappearance of ditto Opposition of Neptune and Sun			8 27·8
Mon	29	2 21	☾ Moon's Last Quarter			8 24·8
Tues	30			3rd Oc. R. 2nd Tr. I. 3rd Ec. D. 1st Oc. D. 2nd Sh. I.	4 36 4 45 5 50 57 6 34 6 51	8 21·9
Wed	31			1st Sh. I. 1st Tr. E. 1st Sh. E.	4 50 6 6 7 8	8 19·1
NOV. Thur	1					8 16·1

THE FIVE INNER SATELLITES OF SATURN.

Elongations of the five inner satellites or their longitudes reckoned from upper conjunction, for 8h. G. M. T., expressed, not in degrees, but in decimal fractions of a revolution.

8h.Gr.	Mi.	En.	Te.	Di.	Rh.
Oct.					
1	·980	·322	·703	·637	·600
2	·041	·052	·233	·002	·821
3	·102	·782	·763	·368	·043
4	·164	·512	·292	·733	·264
5	·225	·242	·822	·099	·486
6	·286	·972	·352	·464	·707
7	·347	·702	·882	·830	·929
8	·408	·432	·412	·195	·150
9	·469	·161	·942	·561	·372
10	·531	·891	·471	.926	·523
11	·592	·621	·001	·292	·815
12	·653	·351	·531	·657	·036
13	·714	·081	·061	·023	·258
14	·775	·811	·591	·388	·479
15	·836	·541	·121	·754	·701
16	·897	·271	·650	·119	·922
17	·959	·001	·180	·485	·144
18	·020	·731	·710	·850	·365
19	·081	·461	·240	·216	·587
20	·142	·190	·770	·581	·808
21	·203	·920	·299	·946	·029
22	·264	·650	·829	·312	.251
23	·325	·380	·359	·677	·472
24	·386	·110	·899	·043	·694
25	·447	·840	·418	·408	·915
26	·508	·570	·948	·774	·136
27	·569	·299	·478	·139	·358
28	·630	·029	·008	·505	·580
29	·692	·759	·534	·870	·801
30	·753	·489	·067	·235	·022
31	·814	·219	·597	·601	·244
Nov.					
1	·875	·948	·127	·966	·465

To find the elongation for—

6h. add	·912	·939	·956	·970	·982
7h.	·956	970	·978	·985	·991
8h.	·000	·000	·000	·000	·000
9h.	·044	·030	·022	·015	·009
10h. add	·088	·061	·044	·030	·018
11h.	·133	·091	·066	·046	·028
12h.	·177	·122	·088	·061	·037
13h.	·221	·152	·110	·076	·046
14h.	·265	·182	·132	·091	·055

The distances of the satellites from the minor-axis of the ring, expressed in semi-diameters of the planet's equator, are then found with the argument "elongation" in the following table:

elong.	elong.	Mimas.	Encel.	Tethys.	Dione.	Rhea.	elong.	elong.
·000	·500	0·0	0·0	0·0	0·0	0·0	·500	·000
·010	·490	0·2	0·3	0·3	0·4	0·6	·510	·990
·020	·480	0·4	0·5	0·6	0·8	1·1	·520	·980
·030	·470	0·6	0·8	0·9	1·2	1·7	·530	·970
·040	·460	0·8	1·0	1·2	1·6	2·2	·540	·960
·050	·450	1·0	1·2	1·5	2·0	2·8	·550	·950
·060	·440	1·2	1·5	1.8	2·4	3·3	·560	·940
·070	·430	1 3	1·7	2·1	2·7	3·8	·570	·930
·080	·420	1·5	1·9	2·4	3·1	4·3	·580	·920
·090	·410	1·7	2·2	2·7	3·4	4·8	·590	·910
·100	·400	1·8	2·4	2·9	3·8	5·3	·600	·900
·110	·390	2·0	2·6	3·2	4·1	5·7	·610	·890
·120	·380	2·2	2·8	3·4	4·4	6·1	·620	·880
.130	·370	2·3	2 9	3·6	4·7	6·5	·630	·870
·140	·360	2·4	3·1	3·8	4·9	6·9	·640	·860
·150	·350	2·5	3·3	4·0	5·2	7·2	·650	·850
·160	·340	2·7	3·4	4·2	5·4	7·5	·660	·840
·170	·330	2·8	3·5	4·4	5·6	7·8	·670	·830
·180	·320	2·8	3·6	4·5	5·8	8·1	·680	820
·190	·310	2·9	3·7	4·6	5·9	8·3	·690	·810
·200	·300	3·0	3·8	4·7	6·1	8·5	·700	·800
·210	·290	3·0	3·9	4·8	6·2	8·7	·710	·790
·220	·280	3·1	4·0	4·9	6·3	8·8	·720	·780

·230	·270	3·1	4·0	5·0	6·3	8·9	·730	·770
·240	·260	3·1	4·0	5·0	6·4	8·9	.740	·760
·250	·250	3·1	4·0	5·0	6·4	8·9	·750	·750
nf.	sf.						sp.	np.

If elong. is between—

·000 and ·250, the sat. is on the following side, north of ring, moving away.
·250 „ ·500, „ „ „ „ „ south „ approaching.
·500 „ ·750, „ „ „ preceding „ south „ moving away.
·750 „ ·000, „ „ „ „ „ north „ approaching.

Approximate Greenwich mean time of some of the conjunctions of the satellites with the ends of the rings and the limbs of the ball, etc.

f. conj. with following edge of ring.	n. north of ring.
fl. „ „ „ limb of ball	s. „ „
p. „ „ preceding edge of ring.	e. at the greatest eastern elon.
pl. „ „ „ limb of ball.	w. „ „ western „

1877.		Gr. M. Time. h.		
Oct.	1	10·6	Encel.	sf.
	2	9·6	Encel.	nf.
		11·8	Dione.	nf.
	3	8·0	Rhea.	nf.
		11·3	Mimas.	e.
		11·9	Encel.	np.
		12·7	Dione.	sf.
	4	10·0	Mimas.	e.
		10·9	Encel.	sp.
		? 12·2	Mimas	sf.
		13·8	Tethys.	sf.
	5	8·6	Mimas.	e.
		10·2	Rhea.	shadow on disc.
		? 10·8	Mimas.	sf.
		12·5	Tethys.	np.
		13·2	Encel.	sf.
		14·2	Rhea.	sp.
	6	7·2	Mimas.	e.
		? 9·4	Mimas.	sf.
		11·1	Tethys.	sf.
		12·0	Dione.	spl.
		12·2	Encel.	nf.
		14·3	Dione.	sp.
	7	? 8·0	Mimas.	sf.
		9·6	Tethys.	np.
		11·1	Rhea.	np.
		11·7	Tethys.	npl.
		13·9	Rhea.	npl.
		14·5	Encel.	np.
	8	? 6·6	Mimas.	sf.
		7·0	Encel.	sf.
		8·4	Tethys.	sf.
		10·5	Tethys.	sfl.
		13·5	Encel.	sp.
		13·6	Tethys.	spl.
	9	6·0	Encel.	nf.
		7·1	Tethys.	np.
		8·0	Dione.	sp.
		9·2	Tethys.	npl.
		14·2	Tethys.	nf.
		14·3	Mimas.	w.
Oct.	10	5·7	Tethys.	sf.
		7·8	Tethys.	sfl.
		8·3	Encel.	np.
		8·9	Dione.	np.
		10·8	Tethys.	spl.
		13·0	Mimas.	w.
	11	7·3	Encel.	sp.
		11·5	Tethys.	nf.
		11·6	Mimas.	w.
		? 13·8	Mimas.	np.
	12	8·0	Tethys.	spl.
		8·7	Rhea.	nf.
		9·6	Encel.	sf.
		10·1	Tethys.	sp.
		10·2	Mimas.	w.
		? 12·4	Mimas.	np.
	13	8·6	Encel.	nf.
		8·8	Tethys.	nf.
		8·8	Mimas.	w.
		? 11·0	Mimas.	np.
		13·6	Dione.	nf.
	14	7·4	Mimas.	w.
		7·4	Tethys.	sp.
		8·4	Rhea.	sfl.
		? 9·6	Mimas.	np.
		10·9	Encel.	np.
		11·1	Rhea.	shadow on disc.
		11·4	Dione.	sf.
		12·1	Rhea.	sp,.
Oct.	14	13·7	Dione.	sfl.
		14·9	Rhea.	sp.
	15	6·1	Tethys.	nf.
		? 8·3	Mimas.	np.
		9·9	Encel.	sp.
	16	11·8	Rhea.	np.

1877.		Gr. M. Time. h.		
Oct.	17	7·4	Dione.	sfl.
		10·6	Dione.	spl.
		11·3	Encel.	nf.
		13·0	Dione.	sp.
	20	6·6	Dione.	sp.
		10·4	Mimas.	e.
	21	7·3	Encel.	np.
		7·6	Dione.	np.
		9·1	Mimas.	e.
		9·4	Rhea.	nf.
		9·9	Dione.	npl.
		? 11·3	Mimas.	sf.
		13·5	Tethys.	sf.
	22	6·3	Encel.	sp.
		7·7	Mimas.	e.
		? 9·9	Mimas.	sf.
		12·2	Tethys.	np.
	23	6·3	Mimas.	e.
		6·4	Rhea.	sf.
		? 8·5	Mimas.	sf.
		8·7	Encel.	sf.
		9·0	Rhea.	sfl.
		10·8	Tethys.	sf.
		12·0	Rhea.	shadow on disc.
		13·9	Rhea.	spl.
	24	? 7·1	Mimas.	sf.
		7·7	Encel.	nf.
		9·1	Dione.	nf.
		9·5	Tethys.	np.
		11·6	Tethys.	npl.
	25	8·1	Tethys.	sp.
		10·0	Encel.	np.
		10·1	Dione.	sf.
		10·2	Tethys.	sfl.
		12·4	Dione.	sfl.
		12·6	Rhea.	np.
Oct.	26	6·8	Tethys.	np.
		8·9	Tethys.	npl.
		9·0	Encel.	sp.
	27	7·6	Tethys.	sfl.
		11·3	Encel.	sf.
		12·1	Mimas.	w.
		12·6	Tethys.	sp.
	28	6·1	Dione.	sfl.
		6·2	Tethys.	npl.
		9·3	Dione	spl.
		10·3	Encel.	nf.
		10·7	Mimas.	w.
		11·2	Tethys.	nf.
		11·7	Dione.	sp.
	29	7·8	Tethys.	spl.
		9·3	Mimas.	w.
		9·9	Tethys.	sp.
		? 11·5	Mimas.	np.
		12·6	Dione.	np.
		12·6	Encel.	np.
	30	7·9	Mimas.	w.
		8·5	Tethys.	nf.
		? 10·1	Mimas.	np.
		10·2	Rhea.	nf.
		11·6	Encel.	sp.
	31	6·6	Mimas.	w.
		7·2	Tethys.	sp.
		? 8·8	Mimas.	np.

A. M.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

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To Feb., 1879.
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The Astronomical Register.

No. 179. NOVEMBER. 1877.

OPTICAL SPECTROSCOPY OF THE RED END OF THE SOLAR SPECTRUM AT A HIGH ALTITUDE IN THE SKY.

PART III.—OBSERVATIONS AT LISBON.

Violently up and down hill is the city of Lisbon, excepting only the winding strand along the Tagus shore, and the ominous-looking valley of the great earthquake of 1755, the grave of 30,000 citizens. Ominous still; for the line of that valley is more marked than concealed by the rectangular blocks of renewed street architecture hastily run up by Count Pombal to fill the vacancy; and, with all the modern houses, so engineered it is said, as to keep up the floors and roofs, though the filling-up masonry of their walls may fall, when the next equally powerful shock shall take place. But that calamitous valley runs northward from the river's bank, near the eastern end of the city and just short of the old Roman buildings at the foot of the castle hill, while all our attention on the morning of July 11 was directed westward, driving right through the Lusian metropolis, even out at its western, or Alcantàra, gate; and then partly by the Belem road, reaching a cross lane which conducts between winding garden walls to the south-west entrance of the Ajuda Park, "Tapada de Ajuda," in the midst of which regal and carefully secluded domain stands the Royal Observatory of the country.

Much to be distinguished from the educational astronomical observatory of the Polytechnic school, on one hand, (though that is an extremely well furnished and fitted observatory, with three domes, two meridian, and other library and laboratory rooms), and on the other hand from the meteorological observatory of the Infante Dom Luis, in the heart of Lisbon—is this western Royal Observatory thereof; for it is devoted to high astronomy and the development thereof, alone. Its building is not indeed quite finished yet, but is growing day by day into full condition for admirable effectiveness presently, under the architectural supervision, as well as the directorial science, of its most able and devoted Astronomer-Royal of Portugal, Frederico Augusto Oom.

The trees in the park are small, but classical and rather wildly picturesque, being old olive trees; and they are scattered about as it were, thinly and at random, leaving large expanses between of a rather luxuriant, strong growth of tall waving oat-grass, gigantic thistles with flowers of various colours, creeping convolvulus in a tangle with blue flowered lupins and legumes of various kinds. But now, in July, they are almost all reduced to a half state of yellow hay, while every tree-trunk is dotted, sprinkled, nay, half covered with white-shelled snails, increasing in number from the ground upwards, until just under the outspring of the branches they cluster by the hundreds and thousands, or as closely as they can wedge together, like barnacles on a tidal sea rock; so that you lose all sight of the vegetable bark, and survey only a curiously bossy, enamelled surface of the snail shells; generally white, but enlivened here and there by a few that are yellow, brown, or bluish, the animals inside keeping themselves to or within themselves most closely.

The aspect was strange, and the poor creatures had perhaps, from what we saw of others in crannies and clefts in a wall, got up there to try to escape out of the burning rays of the sun, the real sun of Lisbon. But we cannot delay on mollusc life now, for that sun *is* shining gloriously, and here is Mr. Oom come down from superintending his many workmen at the grand Observatory structure itself, to receive us at the separate pile of houses which he has erected as residences for himself and future assistants.

These buildings are quite new, and in a suite of their rooms never yet occupied, and with a southern exposure, Mr. Oom had already placed all of our instrument boxes in order. Wherefore it was a comparatively short work, in that abundant space and on those clean floors, to unpack and set up everything, re-adapt the black curtains, push forth the heliostat, whose working by two long handles was then undertaken by my Wife (and very patiently as well as successfully, considering the bad action of one of its movements, which had been a cheap affair borrowed from a foreign solar microscope), and then a whole series of micrometer observations was obtained of lines in the ultra red of the high-sky solar spectrum.

Everything was thus tested in the only undeniable manner as being in place and ready for work. Mr. Oom, too, came and examined, and though a few little thunder-clouds then began to pass before the sun, and convert our dark room for a time into a picture forming camera-obscura, these presently vanished, and we obtained quite as much of measured result as there was any occasion for on that first day. Then followed a delightful little luncheon with Madame Oom in her apartments, and after that we all, both Mr. and Madame Oom and their son, already distinguished for mathematical attainments at college, adjourned to the instrument room to see the spectroscope put through all its dispersion powers on the living sun itself. And though that orb was now declining towards the west, and in so far of no more use that day for our own purposed high-sun spectroscopy only, he was quite high enough for general experimentation to other observers, and seemed rather to reinforce than fade in heat and glaring splendour as the hours passed away.

"May we really hope to have more days as fine as this?" we asked of Mr. Oom. "Every day," answered he, on ordinary Lisbon summer experience, "will be just like this day, *except its clouds.*" And they, as already indicated, had been of only a few minutes' duration.

At that rate, then, we must be careful not to be troubling Mr. Oom too much and too often. Wherefore he gave the keys of that part of the house into our hands, so that we could let ourselves into the instrument rooms at any time without asking further leave or help from anyone; and Madame Oom in a very short space of time supplied so many tables and chairs, and comforts and conveniences for writing and reading, as well as observing, that we quite longed for the next day to begin.

It did begin at last, in its own good time, that 12th of July, so desired by us, who had in the meanwhile returned to the Braganza Hotel—but *how* did it begin?

To our horror, with densely fogged sky and thin rain. The latter ceased as the day drew on, but the fog clouds remained, covering every part of the heavens so thickly and completely that not a particle of sun shone through the livelong fourteen hours, or from theoretical sunrise to an equally unseen sunset.

Of course we did not trouble worthy Mr. Oom at all, that day; and he had, no doubt, a good return of work from his various artificers; while we had a very fair bout with certain proof sheets sent to us every week by book post from 56, Ludgate Hill, London.. After dark we were taken to see a people's fair in the suburbs, close under the water-reservoir of the Roman-like aqueduct, which Portuguese hereditary Latin taste prefers to iron pipes; but the fog continued, the attendance was poor, and the actors at a booth theatre were smoking cigarettes all the time they were on duty.

July 13 came. Barometer = 29·99, Thermometer = 71°·5, but the sky again utterly cloudy—so more proof sheets from Ludgate Hill were looked over.

July 14. Barometer rather higher, viz., 30·05, and the temperature at 7h. 30m., a.m., = 71°·3, but the sky again entirely clouded. Proof sheets all day long.

July 15, Sunday. Sky not altogether clouded over; but we could not disturb Mr. Oom on his one day of weekly rest, for merely a small chance of seeing something indifferently well.

July 16. Another clouded, proof-sheet, day, and all day through. Posted three packages to Ludgate Hill in the evening, near the poetical square of Camoens; whose grand statue, supported by his friends, is surrounded by the gracefully drooping pinnated foliage of the bright green pepper trees; but to-day it was seen in the midst of misty rain, muddy streets, consternation of the oldest inhabitants at such unheard of weather, the occupation of the Gallegos (the Spanish water-carriers of Lisbon) gone, and prostration of all our spectroscopic hopes.

So ended July 16; the earth turned, and on came July 17. We looked out mournfully, as had of late become usual, somewhere about 7h. a.m.—but now, can we believe our startled eyes? for not a speck of cloud was anywhere visible! The whole heavens were blue, blue as lapis-lazuli, except only where the yellow sun was radiating forth to a happy universe.

Did we not, therefore, speedily betake ourselves out to Lisbon's Royal Observatory that morning; and how the sun shone there, continued to shine, blaze, rule the whole creation, all the time; and the instruments were worked until 3, 4, 5h. p.m. After which, on drawing in the heliostat, no smoke particles, no dust had fallen on the silver-on-glass reflector, such is the virtue of observing in a royal park, far away from any public roads, and either manufacturing or domestic chimneys; and as we returned to the Braganza in the evening, we further looked around and around on every side, but positively not the smallest pellet of cloud or vapour was to be seen anywhere.

July 18 was just such another cloudless day of perfect, smokeless, unimpeded, unmitigated sunshine from morning to evening; and we utilized all its circum-meridian portion as before with the largest dispersion power the spectroscope possessed, and with a degree of security in the results we had never anticipated.

July 19. Another day still of perfect solar weather of intensest radiation. Bar. = 30·00, temperature at 8h. a.m., = 75°·8. Splendid day's spectroscoping at the Royal Observatory in the Ajuda Park, says the note-book. Home in the cool of the evening between 7 and 8 p.m.

July 20. Positively another day, as hot, as burning bright, as crystal clear; not a speck of cloud all day long, and no smoke at the Royal Observatory, says the note-book, so that a most satisfactory series of solar spectroscopings was again secured.

July 21, Saturday. Well, a few small cirro-cumuli clouds appeared at last, but we could afford that, for the real work was done; finished with; and this day was to be devoted to packing up for the return home, after Mr. Oom should have critically tested some of the more crucial of our observations by his own independent measures, and should have introduced his two assistants to both ourselves and the spectroscope. He is eventually to have four assistants, but up to three days ago, though loaded with all the architectural, as well as astronomical, work, he had only one proper observatory aid.

So everyone saw the whole of the phenomena, under each degree of dispersion, examined the arrangement of the apparatus, and tried the mode of using it. And then, before evening, everything was packed up once again into its travelling cases, ready for removal.

On Monday that removal of the cases to the Braganza was effected, with a view to freedom for starting whenever the return steamer should arrive. But that was not expected for four days yet, and as the last steamer of the line, a fortnight previous, had been three days late, we ventured to look forward to nearly a week of unoccupied holiday-time, wherein to study Lisbon and the Lisboans, for their own sake and our instruction: while the first part of those days was to be given up to the chief of all the buildings in our eyes, the Royal Observatory itself. And for this, among other pleasant reasons, that Madame Oom would graciously insist on our coming out, after the labours of the past week, to what the Royal Society of Edinburgh, after its session of work is over, calls an unlimited dinner; *i.e.*, a dinner not curtailed by any closely following scientific meeting; and Mr. Oom had still much to show us connected with the progress of his grand Royal Observatory, some seven times the size of the poor, and almost perishing, Royal Observatory of Scotland.

We, too, being just as anxious to learn all the Portuguese astronomer's various methods and improvements, duly made all arrangements for driving over there on July 24, and were counting on them happily, when, just before stepping into the carriage, up came in hot haste a breathless messenger from Signor Pinto Basto, the agent of the Pacific Steam Navigation Company, with the multifariously astounding news:

"The 'Cotopaxi' has come in from Brazil three days *before* her appointed time, and will sail for England this afternoon at 4h. p.m. She is not in sight from here, being obliged by Board of Health regulations, as an arrival from possible yellow fever-stricken tropical regions, to anchor in the mouth of the river off the Castle of Belem. But she is there, and Mr. Pinto Basto will put you on board in his little tug steamer which will be brought up off the pier in front of the Braganza Hotel, at 3h. p.m., when you must join it punctually, with all your boxes. You will have a little difficulty in so doing, for to-day is the great political fête of all modern Portugal, the anniversary of the commencement of its present constitution. The whole city, high and low, has already given up business of every kind, and is holiday-making; while all the country-side is streaming into Lisbon in unexampled crowds to see the king and court unveil the statue of the late chief political hero of their side of politics, the Duke of Terceira, as the crowning event of the day. That statue is close to the pier where a boat will come for you: and as the grand unveiling is to take place just at the instant when you should be going on board, there will be a pretty considerable crowd for you to push through."

And so in good truth there was. The streets thereabouts seemed already almost as full as they could hold of standing human beings, and yet more and more were arriving every succeeding minute, pouring in by every avenue from all parts of the compass, and always converging towards the very spot on the Tagus shore where we ourselves were bound to embark.

But the Portuguese are an eminently good-natured, kindly, well-mannered style of people. We had gradually learned that, during the last month, by studying them individually; and now the conclusion was found to be as true of them collectively, even on the largest scale, and at the most exciting instant. How well we were led through the crowd by the waiter Francisco, who further remained by us all that day, until we actually climbed the sides of the compromised and compromising ship "Cotopaxi," in the dusk of the evening off Belem; and how good-humouredly the crowd, both men and women, did bear being punched in the ribs, and pushed out of their respective coigns of vantage, just at the ecstatic moment too of their believing that the king would raise his arm aloft, and the curtain fall suddenly from the never yet publicly beheld statue, and that they would lose that precious sight, for which they had sacrificed so much already. These good souls, too, being thus unceremoniously pushed out of their places, not for the momentary passage of one or two persons only, but for half a dozen or more in winding line, and all of them with struggling gait, carrying big bags or boxes more than shoulder high! So Roman, so sturdy in some things; so mild, so complaisant, so touched with true charity to their neighbour in other things, is this worthy Lusitanian people; even as we actually saw a signboard that very morning, inscribed "Julius Cæsar 0—0, maker of caps and bonnets." That, indeed, had something of the comical and accidental about it; but the behaviour of the free and independent crowd of myriads, met there that day in united strength to celebrate their own recent political, and even military, triumphs, exhibited in the most unmistakeable manner, the gentleness of true gentlemen to the few or the weak, combined with the martial qualities, if required on a great occasion, of as good soldiers as ever fought under Wellington himself.

So the dense crowd was got through, permeated in some way or other, at last, even to the river's brink; but there we found another crowd densely afloat on the waters; every available boat there being crowded with earnest, anxious spectators, and all of them in an agony of instant hope looking out for the king's hand to be put forth, and for themselves to see the curtain in the very act of falling from the new statue; a thing it could never do again. But time was pressing with us: so both we and our packages had to trample almost roughshod from boat to boat of that wedged in, floating, phalanx of vessels of all sizes surrounding the pier, until we could get to our own boat, which was outside them all, and ready to convey us to the tug lying in deep water beyond;—if indeed it has not already started to take its much required supplies of fresh provisions to the hungry "Cotopaxi" at Belem.

We were thus just in the act of successfully passing through the last boat-load of spectators, all of them looking landward, and ourselves seeing clear water before us,—when, suddenly the air shook with that peculiar tremor which only the one universal shout of tens and hundreds of thousands of men can give, on being acted upon by some grand, united impulse. The king had done the moment's deed! With a flash, the curtain had vanished; and now the Duke of Terceira was seen over the heads of the people in yellow bronze, drawing his patriotic sword once again. Whereupon the military bands struck up, the cannons fired, volleys of rockets filled the air with detonating crackers; the whole of that vast multitude relieved itself of its long pent-up, wound-up, almost bursting, high pressure intensity of expectation, in a deep gasp of thankfulness;—and then began the criticisms. When many a good country woman and man too, on those barges of queer Roman built bows, seemed, if we could judge by their countenances, to look slightly disappointed, and trim their features, and say,—"Ah! well! It's all very well, you know, it's all very well; but when they *were* about it, might they not have made the statue a little bigger?"

From the deck of the "Cotopaxi" (Captain Graves) that evening we had a last distant view of the Royal Observatory of Lisbon, where we had been entertained so kindly, and where we were also to have spent that very afternoon. Opposite to it the

Palace of the Ajuda stood up nobly against the twilight of the north-western sky; and below it, far below in the shadowy gloom of the growing night, and, unhappily, too much neglected in these days of the civil power, more than any ecclesiastic, infallible, and dry political economy rampant, could just be recognised the venerable Church and Convent of St. Jerome, at Belem. A worthy historical landmark; having been erected more than three centuries ago, by the good King Manuel, on the site of the humble little chapel wherein, in the year 1497 A.D., the immortal Vasco de Gama and his intrepid companions, when they had prepared everything else for embarking on their unprecedented and most adventurous voyage—which, with such a beginning, resulted in the discovery of the ocean way to India, and the making of the fortunes of their country,—spent the night, not in rioting and drunkenness, not in feasting, speechifying, and rounds of applause, but in vigil, fasting, and Christian prayer; asking the pardon of God on their sins, and beseeching His blessing on their perilous undertaking.

THE END OF IT ALL.

But now before the night grows too dark, or that other night comes wherein no man can work, let me attempt a word of explanation to those whom it may concern, as to what we were enabled to do in solar spectroscopy, why it was undertaken, and how it was done, during these four glorious, spotless days of superb, striking, actinic sunshine when our instruments (the movement of the heliostat alone excepted) worked so well in the Royal Observatory house of the Ajuda Park, to the west of Lisbon and the smoke thereof.

The one great and almost perfect map of the solar spectrum, incomparable in its exactitude, and universal as well as absolute in its scale of "wave-lengths," and with which not even the large map of Professors Bunsen and Kirchoff can compare, is that of the late M. Angstrom, of Upsala; and he made it his life-labour to so large an extent, because he was early gifted to see that all the spectrum analysis of every modern observer, on every element of terrestrial chemistry, both simple and compound, gaseous, liquid and solid, should be made to depend on, fit into, and be placed, known, and identified by the spectrum of the one great, natural, and most active chemical laboratory and furnace-fire of the whole solar system, the sun itself. Thence it was that, after years of toil, appeared for all science, of all nations, Angstrom's now celebrated and necessary Chart of "the Normal Solar Spectrum;" engraved too as exquisitely as it had been most scientifically observed.

But that chart did not begin, for the red portion, at the very beginning of the red end of the solar spectrum; where, no doubt, the light *is* exceedingly faint, but it commenced considerably within that red end, at a place marked naturally by the band of lines anterior to little *a*, and where the general illuminating light of the continuous spectrum has grown much stronger, as well as brilliantly verging to a scarlet red. Hence between that little *a* band, and the real, faint, ultra, or maroon-coloured red end of the whole spectrum, there was a comparatively unknown, or rather unmapped-on-a-large-scale, region of true solar spectrum; and to map that now, in any style of accuracy at all comparable to Angstrom's splendid treatment of the rest of the solar optical spectrum onward through its yellow, green, blue, and violet portions, appeared worthy of any observer's best efforts.

For myself, however, both individually and officially, there was something still more obligatory. In the year 1872 I had published in the 13th vol. of the *Edinburgh Astronomical Observations*, some little spectroscopic notes, rather than measures, of the red end of the solar spectrum; setting forth, amongst other things, that a certain line called Y in the ultra red, was more visible in a high, than a low, sun: and that simple fact at once raised the said Y out of the herd of mere telluric, meteorological effects into the dignity of a true solar line.

But in the year 1875 the Royal Society (London) stepped into the arena, and virtually declared that the above was not the fact, by publishing a double map of the red half of the solar spectrum; wherein, although Y appeared conspicuously in a low sun, there was nothing whatever of it in a high sun. That map was, moreover, the Society's second map of the kind, made for them by a good professional observer in India, not only stationed in a pure atmosphere on the Himalaya mountains, but furnished by the Royal Society with the *second* spectroscope they had sent out to him, after having had it specially constructed for precisely such standard solar work.

But was even their second spectroscope very appropriately arranged for those important observations?

"IT MUST HAVE BEEN," the world replies, "if it was made to the express order, and under the auspices, of so elevated, worshipful, and unapproachable a body as '*the* Royal Society:' a society some 600 Fellows strong, enormously rich, ruling autocratically all science in London, and settling the Government grants as well."

At an earlier period, I should have, perhaps, like anyone and everyone else in the country, simply and immediately succumbed; and allowed thenceforward the Y line to be degraded to telluric level, because the Royal Society had said so. But the end of

1874 had seen that whole Society tacitly homologate certain too remarkable acts of their president and council during that year, in publishing and republishing an erroneous as well as degrading statement, and preventing the correction thereof appearing, in both of the Society's publications, touching a far older, grander, more soul-exciting subject even than the solar spectrum, or any other problem yet known throughout all the range of human science; viz., the true length of the base side, as the first step in present day interpretation, of that most primeval, and even *miraculous,* monument of number, weight and measure, the Great Pyramid, still standing in the central land of all the man-inhabited surface of the earth.

That particular act of the Society has now, indeed, passed into the hands of others far abler than myself to deal with it; and the 600 members may possibly live to learn, even in spite of their own edicts and the support of their secret committees, that—

> "Truth, crushed to earth, will rise again,
> The eternal years of God are hers!
> But error wounded, writhes in pain,
> And dies amid her worshippers." *

But the other act of the Society, viz., its last effort in expunging the line Y from solar, red end, high sky spectroscopy, was one where it should be both within my compass, and even be part of my duty, to enquire into any antagonistic statement to my previously published one; as well as to give the world new instrumental results, extra free, if possible, from mental bias. Moreover, as this particular question about the line Y was necessarily included in the independently proposed mapping, for a high sun, of *all* the portion of the red end of the spectrum not contained in Angstrom's larger map—my course was perfectly clear. I went, therefore, with my Wife, as soon as we could afford the expense, to a southern country, where the sun should be visible at noonday at an undoubtedly *high* angle of altitude: and, armed with a newly-constructed spectroscope, 1st of sufficient dispersion power; 2nd of accurate measuring apparatus; and 3rd of appropriate optical appliances, in my private judgment, for the particular observation,—proceeded to map all the ultra red end of the spectrum which I could see, when the sun was at any time above 70° high,—beyond Angstrom's first entered record in his grand Solar Normal Chart.

Now, of "the dispersion power," I have already spoken in Part II. (see *Astronomical Register* for October). Of "the accurate measuring apparatus," I may here simply state that with the greater part of the rest of the instrument (including prism 5, whose light-power in the very extreme of the ultra red end of the spectrum was unmatched) it was constructed for me by my old friend of the French Exposition days of 1855, M. Jules Salleron, now a maker of "instruments of precision," at 24, Rue Pavée au Marais, Paris; and constructed, I am happy to acknowledge, with genius, force, and remarkable expedition. But "the appropriate optical appliances," what were they?

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* See page 305 and page 596 of *Our Inheritance in the Great Pyramid*, 3rd edition. Daldy, Isbister & Co., Ludgate Hill, London. 1877.

These: However narrow may be a spectroscope's slit, the amount and spectral variety of light that rushes into it from the sun, if any at all passes, is something so tremendous, that the whole field is filled with false glares arising from side reflections. These, too, are felt to be more and more overpowering, in proportion as the particular part of the spectrum under observation is fainter than other parts which may be outside the field of view. Now, the red end being the very faintest portion of all its region of the spectrum, it gets entirely swamped by the abundance of false light from the orange, yellow, and other regions, unless some special and very effective method of stopping them out, while admitting the ultra red, is adopted. The usual plan hitherto for that purpose has been to employ coloured glasses. Such, too, I used largely; until finding them to be attended with certain other disadvantages, I obtained from M. Salleron a large anterior prism of white flint glass, for receiving the beam of light direct from the heliostat, and converting it into a coloured spectrum streak, before it reached the slit of the spectroscope proper. In this manner, exactly according to the part of such preliminary spectrum thrown upon the slit, so were the rays which, passing through it, entered the subsequent prisms; and, excepting a trifling percentage caused by motes in the air, microscopic imperfections of the glass, &c., &c., no other rays from any other part of the spectrum were allowed to enter at that time.

CONCLUSIONS DEDUCED.

When such a preliminary spectrum was allowed to approach and then pass over with its red end, the spectroscope slit, the following was the order of events observed:

(1) Light first appeared at one definite spectrum place (51·162 of my micrometer, see Plate 3) in an otherwise almost entirely black or dark field of view.

(2) After a few seconds' further approach of the exterior preliminary spectrum, under the influence of the sun's diurnal motion, the admitted light became stronger; and the originally first spectral light-gleam of the undefined continuous part of the true solar spectrum, in the telescope's field of view, expanded both ways, or reached back to 51·704 as well as forwards to about 50·500.

(3) After that, with all further progress of the preliminary spectrum over the slit, the growth and motion of the light in the field of view, or the development of the continuous solar spectrum there, proceeded *towards* the yellow, green, blue, and violet only.

(4) In that direction of *greater refrangibility*, the visible light of such continuous spectrum portion or patch went on, from the place of its maroon-coloured beginning, shooting forward into the yellow like a comet with its head resplendent in the perihelion solar rays; and every moment becoming brighter and brighter, *because* entering a naturally more brilliant part of the sun's continuous-spectrum region. But also, if you please, note the following *simultaneous* phenomenon, viz., that when this visible travelling head of light came to each spectrum place of any well-known solar Fraunhofer line, it seemed to go behind it, exhibiting the line thereby most distinctly, and proving it undeniably to be something already in existence, and even long since established there.

(5) Precisely in this way, too, did the light at 48·600 of the micrometer act on the line Y, which thereby and thereupon appeared to us, day after day, as thick, solid, black, well-defined, and as constantly present in that manner in a high-sun, even our very highest sun, spectrum as that most solar of all the solar lines, the C line itself—see Plate 2. And all this habitude and physiognomy of Y was observed most surely, notwithstanding Y's total absence from the Royal Society's high-sun spectrum view of 1875.

(6) Finally, passing over several, almost infinitely fine, lines—for which examine our plates 3 and 4, and for the power of seeing and measuring which I am chiefly indebted to Mr. Adam Hilger's admirable prism 6, and his bright reference-line eye-piece—we come at last to the first and earliest spectral line from the red-end, shown in the Royal Society's high-sun spectrum, viz., the great A line; and I would simply request anyone with an eye in his head to look at the two pictures, the Royal Society's and mine (especially referring for theirs from my approximate copy of it only, to the original in their own *Philosophical Transactions*); and then say whether the Royal Society on their second, or 1875, occasion of attempting the work, has done its proper duty to science and the modern world, in publishing at that date so bald, so perverted an idea of the most glorious, most extensive, most marvellously composed, most intricate, yet most symmetrically arranged of all the lines in the whole solar spectrum, from one end of it to the other, viz., the great and composite A line and its preliminary band of lines. Both of these have been assumed hitherto as being of telluric origin only; but if so, why so grandly, blackly, prominently visible in a high-sun spectrum, *observed with proper precautions*, when B and *a*, acknowledged

telluric lines (first announced and proved to be such, I believe, by myself on the Peak of Teneriffe in 1856), and "little *a* also," nearly fade away under exactly similar circumstances—as see again our Plate 2 of October's, and Plate 4 in the present, *Astronomical Register*.

And yet, whatever my drawing of A may show above that of the Royal Society in 1875,—the real A line in nature (and in a high sun, remember, not when low and oppressed with base absorption effects of our own atmosphere, its impure vapours and cold, wet gases) is almost infinitely richer still. So that this majestic "great A" line may perhaps some day open out to future observers, furnished with still better spectroscopes, even as the two little Magellanic clouds of early navigators under southern skies did to Sir John Herschel, when that admirable observer as well as great mathematician, turned his twenty-foot reflecting telescope upon them, and found double-stars, clusters of stars, systems of suns, and gigantic nebulæ almost innumerable, within their apparently micro-cosmic bounds. P. S.

SATELLITE OF MARS.

BY WENTWORTH ERCK.

On the 2nd September I, for the first time, looked for the satellite, and at once saw a faint object at about 290, and distance from the limb rather more than two diameters of the planet: during the short time I was able to observe it the motion resembled that of a satellite moving in the direction in which the position of a double star is reckoned.

It subsequently appeared that I was mistaken in supposing this to have been the satellite; but yet the observation of this star has proved most valuable: for its position with regard to the planet was similar to that in which the true satellite was afterwards observed, whilst its apparent brightness was, as nearly as I could judge, the same as that of the satellite itself. Thus, when the star was left behind by the planet, we had an isolated standard of comparison whereby to estimate the brightness of the satellite irrespective of the light of its primary.

My observations of the satellite were as follows, and at the time of making them I had no knowledge whatever of its place. The times are Greenwich sidereal.

September.	Pos.	Remarks.
3 23 0	64°	Distance from the limb put at 3 diams.
4 0		D. estimated at 2 diams.
,, 1	55°	
8 22 35	78°	Cloudy.

15	21	10	257°	
	23	30	243°	
16	1		238°	
25	21	20	270°	Gibbosity conspicuous: very much fainter than formerly.
25	22	20	264°	Sufficient moonlight to read *N. Almanac.*
26	0	35	255°	

I had no difficulty whatever in seeing the satellite when proper precaution was taken.

I used a sexagonal aperture; and, as an eye-piece, a single lens giving a power of 300. The field of view was limited to 2 minutes, and the planet placed just outside the field. I then turned the hexagonal aperture till the satellite was exactly in the centre between the rays; when this was done it was only necessary to read off the position of the rays with a position microscope, and, adding 30°, there was the position of the satellite.

But the estimation of the distance was little more than guess work; the standard of estimation not being in sight along with the space to be estimated.

On every occasion I looked for the inner satellite, but looked in vain. As to the magnitude and brightness of the satellite, I should like to defer my remarks till we shall have had more favourable weather. But one word with regard to the use of large apertures in such cases.

They seem to me to be nearly useless; as, in practice, they are found to illuminate the background against which the faint object is seen. Of course, if the figure and glass were absolutely perfect, or even as good as some smaller ones, then the gain would be great; but, so far as my information goes, such is not the case.

As an instance of the very small aperture with which the satellite was visible, I may mention that I had little difficulty in seeing it steadily, not by glimpses, with an equilateral triangular aperture of which the side was 6 inches, giving an area of 16½ square inches, equivalent to a circular aperture of about 4½ inches diameter: and I am quite confident I could have seen it with a still smaller aperture.

Still, though this aperture may appear small to have shown the satellite in *loco*, I am perfectly certain, judging from the companion star alluded to, that less than one-third of this aperture, that is to say, one-tenth of the area, would have sufficed to have shown the satellite in an isolated field. So that, notwithstanding all our precaution, nine-tenths of the light of the satellite was overwhelmed by the brilliancy of the primary. Having regard to Sir William Herschel's observations, I have been surprised at the persistency of the South Polar snow cap.

Sherrington, Bray: 26 September.

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

JUPITER AND THE MOON.

Sir,—In a paper read before the Royal Astronomical Society in March, 1877, I announced that two terms which had long been known to exist in the perturbations produced by Jupiter in the longitude of the moon possessed co-efficients of comparatively large magnitude. In a paper read before the next meeting I showed that one of these terms was identical with the empirical term of short period whose existence had been demonstrated by Prof. Newcomb. In a paper read before the June meeting of the Society, Prof. Newcomb, whilst confirming my identification of his empirical term with the term of short period due to Jupiter, also showed that the larger and more important term, a term of long period, was strongly indicated by observations.

In August and September, Mr. Richard A. Proctor thought proper to address a series of letters to the *English Mechanic*, at first implying and then explicitly charging me with having passed off the discovery of these two terms as my own, when the discovery had been really made by a correspondent of the *English Mechanic* in the preceding year. I passed over in silence Mr. Proctor's first letter, dated August 4th, 1877, for reasons I need not specify, and because I trusted that this correspondent (who signs himself W. C. P.) would not allow such a charge being made in his name without reading my paper in the March number of the *Monthly Notices*, and so ascertaining for himself its groundlessness. After doing this I made sure he would have repudiated the charge. This correspondent, W. C. P., without doing me the justice of reading my paper, accepted Mr. Proctor's statements without troubling to verify them, and claimed redress. On seeing his letter, I immediately wrote and offered to communicate to the Royal Astronomical Society any statement of his claims he might think proper to make, and promised to use my best endeavour to obtain for it as complete publicity as my own paper had enjoyed. This offer W. C. P. has declined to avail himself of, and he appears disinclined either to reveal his identity or bring any claims he may possess before the Royal Astronomical Society. Since that time Mr. Proctor has published two strong letters on the subject reiterating his charge.

Although strongly tempted to content myself with simply denying the accuracy of the statements in Mr. R. A. Proctor's letters, as I find that the charge against myself has been also made in an amplified form in the astronomical summary of the October number of the *Popular Science Review*, I think it may be as well to give the real facts of the case in the *Astronomical Register*.

In the *English Mechanic* for June 16th, 1876, is a very obscure report of a paper by Prof. Newcomb, which was read before the June meeting of the Royal Astronomical Society. It implies that Prof. Newcomb had discovered some empirical inequalities in the longitude of the moon. In the issue for June 20th, 1876, p. 405, a letter appears from a correspondent, W. G P., asking how far two inequalities due to the action of

Jupiter agreed with those discovered by Prof. Newcomb. These terms he stated to be

$$-1'' \sin. \{ (2 - 2i - c)\, t - 2\,\varepsilon + a \}$$

a term of short period (with an obvious error in the argument), and

$$p \sin. \{ (2 - 2i - 2c)\, t - 2 - 2\,\varepsilon - 2\,a \}$$

About the second, which is a term of long period, the correspondent stated he was not sure, but he thought it was smaller than the first. Immediately afterwards (*English Mechanic*, July 14th, 1876, p. 461) W. G. P. explicitly states that the term of long period is *insensible*, and its value to be only ·35″, but in his next letter (*English Mechanic*, July 21st, 1876, p. 484) he states that ·35″ was a misprint for ·15″ (erroneously printed in this second letter 3″·5 and 1″·5) and in a subsequent letter (*English Mechanic*, August 4th, 1876, p. 535) W. G. P. confirms this by omitting this term of long period from the list of inequalities with co-efficients, greater than 0″·25 discovered by him. The first letter from W. G. P. was followed in the next number (July 7th, 1876, p. 436) by one from a correspondent signing himself W. C. E., who, referring to these inequalities, deduced their value in a remarkable manner. The value of this term of short period he stated to be made up of two portions, that portion due to the direct action of Jupiter to amount to —0″·181, and that portion due to the indirect action of Jupiter to be —0″·873. The term of long period he declares to be insensible, and his method gave —0″·05 for its value. Subsequently this correspondent admits that his value for the term of long period is entirely wrong, and makes it equal to +0″·09. This is practically all which has been said about these two terms by these two correspondents of the *English Mechanic.*

The existence of arguments of this form in the case of every planet in the solar system has been known for years, ever since the lunar theory has possessed its present form in fact, and it is impossible that the discovery of these arguments can be claimed for anyone. What has been stated above proves that the correspondent who signs himself W. C. P. indisputably discovered that the term of short period had a considerable co-efficient. So far Mr. Proctor is right, but the charge he brings against myself is untrue, for in my paper in the March number of the *Monthly Notices* I distinctly state in the most explicit manner that I had been anticipated in my discovery. I did not mention the name of the person who had anticipated me, for the very satisfactory reason that I neither knew it, nor could find it out. His name was unknown even to the editor of the periodical to which he wrote. With regard to this particular inequality, all that I ever claimed was the accurate determination of its real value, and the identification of it with the empirical term discovered by Prof. Newcomb. So far from having led Prof. Newcomb to suppose I had discovered this term, I would draw attention to the fact that Prof. Newcomb has scrupulously refrained from speaking of me as discoverer, but throughout refers to me as having "only pointed it out" as the term discovered by him. In the first letter I wrote to Prof. Newcomb, after seeing his note on my paper, I mentioned that I had been anticipated in my discovery.

It is very different though with regard to the second and more important inequality, the term of long period. What has been stated above proves in the most unquestionable manner that neither of these two correspondents of the *English Mechanic* ever discovered the large value of this term. Nor did they ever draw attention to it as a term which might be of importance. They have both most explicitly stated that it was so small as to be insensible. They have, therefore, not the remotest claim either to being its discoverers, or being the means through which its discovery was accomplished. For I need scarcely remark that no one would dream of calculating the value of a term which has been stated to have been independently investigated, and proved to be insensible by two mathematicians. Mr. Proctor's claim on the behalf of these two correspondents, of the discovery of the large value of this term of long period, is therefore entirely untrue.*

Whilst on the subject, there are one or two more points I may as well touch on. It is implied by Mr. Proctor (*English Mechanic*, Sept. 14th, 1877,) but stated downright in the *Popular Science Review*, that in obtaining my own results I made use of the information published by these two correspondents, W. G. P. and W. C. E., it being only necessary for me to perform some "subsidiary calculations." This is not so. There is simply nothing in the whole *English Mechanic* which could be of the slightest assistance to anyone in determining these inequalities due to Jupiter. About the calculation of the term of long period, the one correspondent W. G. P. has said nothing, and about the other term (whose discovery I did not claim) he has simply mentioned that its integrating factor is $c^2 - p^2$. No doubt this last is interesting if true, but scarcely valuable, and leaving nothing in common with my own results. The other correspondent, W. C. E., has, it is true, given a method for determining these irregularities (*English Mechanic*, July 7th, 1876, p. 426). About this process I shall simply content myself with saying, that it is so utterly inadequate for its purpose, especially as applied to the term of long period, that it is absolutely valueless. That I did not make use of this process is easily shown by the fact that my results differ *in toto* from those derived by W. C. E. from it. In fact in a subsequent letter (*English Mechanic*, July 28th, 1876, p. 510). W. C. E. himself acknowledges its inadequacy for the purpose of determining the value of the term of long period. In fact the particular class of terms depending on the angular distance of the disturbing body from the lunar perigee, to which this term of long period belongs, presents certain peculiarities which as Plana and Pontécoulant observe, render them the most difficult in the entire lunar theory.

The only other information on the subject given by W. C. E. appears to be a description of the well known principle of calculating the lunar irregularities employed under different forms by Damoiseau, Pontécoulant and Lubbock (*English Mechanic*, Aug. 25th, and Sept. 18th, 1876.). This description is quite valueless to anyone not well acquainted with the lunar theory, and to whom it must be perfectly familiar. And in justice to this correspondent, it must be stated that he has admitted as much. (*English Mechanic*, Sept. 21st, 1877.)

Mr. Proctor explicitly states that W. G. P. had given "nine months earlier" than myself the same explanation of the origin of the large value of these two inequalities as I have done in the *Monthly Notices*. This is not so. The only reference of any kind made by either of these two correspondents, is a statement by W. G. P. (*English Mechanic*, Aug. 4th, 1876, p. 535), that the term of short period corresponds with the *evection*, and had $c^2 - p^2$ instead of $1 - p^2$ for its integrating factor. Neither of these have anything to do with the large value of the term. Not a single word has been said by either of these correspondents about the origin of the large value of these terms (in fact, they only knew one of them to be large). I have myself alone pointed out that these two

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Poisson had investigated the long Venus term, before Hansen, but though Hansen knew this he is not accused of piracy for claiming the discovery of the larger value of this term on the ground that Poisson did not find it large.

terms derive their large value from the fact of the near approach to commensuribility between the periods of revolution of Jupiter and the moon's perigee.

The fact of the commensurability of these periods struck me while at work on the Venus inequalities of Hansen a long time back. I then made up my mind to attach the long inequalities due to Jupiter, together with some due to Venus, &c. Prof. Newcomb's paper led me at once to take up the long inequalities due to Jupiter, and I was led to undertake the determination of the value of the term of long period solely by this consideration. The inequality was calculated by a process devised by myself, and which I had previously employed to verify the lunar theory of M. de Pontécoulant. The fact that the coefficient of this term of long period was of considerable magnitude was announced by me to a very distinguished worker on the lunar theory, shortly after the publication of Prof. Newcomb's paper, I having then arrived at a provisional value larger than I subsequently found, from a more extended calculation, to be correct. By the same process I determined the value of some similar terms of smaller value.

So far, then, from deriving assistance from these two correspondents, in discovering the important character of the term of long period due to Jupiter, my work had been provisionally accomplished before this was possible, even had the information which could have been derived been either trustworthy or adequate.

With regard to the credit (if any) which Prof. Newcomb has awarded to me, for "pointing out" that the term of short period due to Jupiter was identical with that discovered by Prof. Newcomb, I would simply remark that I alone took the few necessary steps to show that these two terms were identical, whatever opinion the two correspondents of the *English Mechanic* may have held, and that events themselves have shown that had I not done so to this very moment the theoretical explanation of the term discovered by Prof. Newcomb would have remained unknown to astronomers.

I do not think I need add anything more to the above to show that the charges advanced by Mr. Proctor are unfounded, and I am perhaps foolishly too good-natured to want to add more.

Scientific Club, 7, Savile Row: E. NEISON.
October 9, 1877.

MIRACULOUS VISION.

"—*Κεἰνου γὰρ ἐπιχθονίων πάντων γένετ' ὀξύτατον' "Ομμα.*"—*Pindar.*

With the general tenour of the first page of Captain Noble's remarks on "Anomalies of Vision" I most cordially agree. As regards colour-blindness I believe that a large number of the apparent anomalies in the recorded colours of double stars may be explained in this way, though I am not prepared to go to such lengths as Wilson, who asserts that one person in every eighteen is colour-blind, and that one in every fifty-five confounds red with green. A good deal of this discrepancy is, I believe, due to the great difficulty most people find in describing in accurate language the exact shades of colour they see. For instance, in the well known case of Cor Caroli, one observer may call the large star "light dingy yellow," while in a few moments after with the same telescope and power another can only term it "cream colour," while a third deems it to be "white." In the even more notorious case of 95 Herculis, ♅ calls both the stars "white coloured"; Σ at one time "both yellow," at another

"yellowish green, very red:" Admiral Smyth, "light apple-green, cherry-red"; Lison, "both greyish white"; Knott, "pale greenish yellow, pale rosy yellow"; Talmage, "both white".

At first sight such discrepancies seem inexplicable, except on the theory of an actual change in the stars themselves, but when one finds that less than a month before P. Smyth, at Guajara, called both the stars "greyish white." Padre Secchi, at Rome, recorded *his* impression of their hues as "viridis, rubra;" and that P. Smyth himself within a week after his first observation describes them as "both yellow with tinge of green"; that some years later Higgens calls them "apple-green and cherry-red"; when P. Smyth thinks that one should be termed "yellowish with blue tinge", the other ' slightly reddish yellow"; and finally that when according to Macdonald, at Sydney, both stars are "golden yellow"; Durer, at Lund, writes "vert clair, jaune"; then, I think it becomes evident with what extreme caution the hypothesis of actual change should be received. As regards the apparent superiority of a blue star over a red one, I take it that most people would rate a blue star of larger magnitude than a red one, but I do not think that would prove that the blue star was really the brighter, and I believe that if the companion of δ Cygni, for instance, was red instead of blue, it would be easier seen on a dark night than it would soon after sunset, while as it is δ Cygni is better seen by the majority of observers before sunset than after. Smyth, besides, appears to call nearly ninety-nine out of every hundred small stars, *i.e.* stars smaller than 11m., either blue or bluish.

Some people, undoubtedly, seem to be able to see faint stars far better than others, *e.g.*, one man may find it very difficult to glimpse the companion to Polaris with a 2¼-inch, while another may often see the same star with 1¼-inch aperture of a smaller telescope, but I cannot but think that the perception of planetary workings, &c., is due rather to familiarity with the objects observed than to exceptional acuteness of vision. Mr. Dawes' reputation rests, not upon his having happened to pick up a few faint stars, but upon his almost unequalled series of micrometrical measures; indeed I was not aware that he ever claimed to have an exceptionally acute eye. Still, taking into account all differences of this sort, some of the performances of that "infant phenomenon," Baron von Ertbon's nephew, seem simply incredible, and I should like to devote a few lines to a detailed examination of the ones Captain Noble gives. As regards the Pleiades, there is nothing so very extraordinary in the lad having seen fourteen stars, as several people have done the same thing before now, but to me the most curious part of the story is the boy's exclamation "I see seven large and seven small stars there." I should not have thought that the transition in light between Merope and her cousins would have been so abrupt as this seems to indicate.

I confess I am very sceptical of the alleged new companions of Polaris. Some time has now elapsed since the announcement of their disovery was made by M. de Boë, but I have not heard that they have either been seen or measured by any well known double star observer. It has happened before now, unfortunately, that supposed companions to stars have been seen and measured, in one instance, as no doubt Captain Noble remembers, a long series of apparently unimpeachable measures were made, and the star was also seen in England, but, alas, the companions have turned out to be no companions at all. If such an experienced observer as Otto Struve could err, if Admiral Smyth could enter, not one, nor two, but many stars in the cycle as having companions which do not really exist, I think I may be pardoned a doubt if M. de Boë's companions are not due either to his imagination, or his telescope, or to both combined. I wish Captain Noble had not thought it unnecessary to add what English observers have seen these stars; the only instance I have found is the one recorded by Mr. Slack in the *Astronomical Register* for November, 1876. I know I have looked for them several times with my own telescope, and also with a 6-inch Wray and two exquisite reflectors of 6½ and 8½-inches aperture, but I have never been able to see a trace of any stars nearer than the well known companion.

I believe that Captain Noble is the fortunate possessor of an altogether admirable object-glass of almost the same diameter—I forget at this moment whether it is 4⅓-inch or 4·2-inch—as the 4·25 in Steinheil used by this "gifted young Belgian", and I would suggest that if he looked at β Equulei he would not find much difficulty in seeing the little pair divided in his instrument; "70 Ophiuchi with two minute companions" is a trifle vague, but I suppose that the two meant are those noted by Secchi: I see these and three or four others in my 6½-inch Calver, but none are near enough to be called "companions" in the strict sense of the word. But now comes the rub: I suppose by "the companion of μ^2 Herculis" we must understand that the Baron von Ertbon's nephew sees μ^1 Herculis double. μ^1 Herculis is a binary, and we are not told when it was observed, but I take it to have been about last September. About the middle of that month I turned my telescope on μ^1 Herculis, and was delighted to find, not that I could see it double, but that I could see the star elongated, and could make a fair estimation of the distance and pos. angle; the former I guessed to be about 0″·8. Just a month before Prof. Hall measured this star with the 26-inch, at Washington, and gives the distance on August 14th, 1876 as 0″·72, and on August 16th as 0″·71, so I think we may consider that the distance last September was about 0″·7. Smyth remarks that "all my endeavours to pry into the duplicity of B—itself so small—were *re infectâ*," the distance between the two stars being at the time about 1″ ¾ ±, and his instrument of course the 5·9-inch Tulley. It does seem rather strange, therefore, than an observer who does not seem to have had very much experience with double stars—I do not know how much lies hidden under Captain Noble's "&c., &c.," so I speak under correction—should see this very difficult star double with a 4¼-inch, when the distance between the two stars was only 0″·7. But this is a mere trifle to the next observation recorded by this wondrous boy. With his left eye he sees 178 Delphini quadruple! I think it is not at all impossible to see the little star in this group in Smyth's 16-inch, with the aperture used, indeed I have found it easy in bright moonlight with 6½-inch, but I believe it to be perfectly impossible to divide the close double with anything less than 8-inch. There is, by the way, a fifth star in the asterism, which I found a few weeks ago, but, as it is a faint object with an 18¼-inch equatorial, I do not think it very probable that anyone could see it with a 4·25-inch. P. XX. 177 Delphini is not a binary, and the distance and pos. angle of the two stars composing the most difficult of double stars have not varied since it was first discovered in October, 1840. A rough mean of three sets of measures by Dawes, one set by O. Σ, and one by Newcomb, gives P = 210°·95, D = 0″·59. The last measures I have (those of Professor Newcomb, in September, 1874) make P = 210°·6, D = 0″·57; a result that does not differ very much from the general mean. It will be remembered that the stars are about 8½ and 9 mags. of Σ.'s scale. Is it possible for anyone to divide these two stars with a 4¼-inch o. g.? I know I have never been able to do so with a very fine 6½-inch mirror, and I considered three or four years ago that the pair was considerably more difficult then γ^2 Andromedæ.

Mr. Ward, I believe, also sees stars without rays, and I have heard of other cases. If this extraordinary child can really *count* the separate larks in the flocks before ordinary mortals can see a trace of the flocks themselves (which is, I suppose, what Captain Noble means), I can only suppose that by some means or other he has succeeded in getting hold of a pot or so of the miraculous ointment the dervish used. Captain Noble does not tell us the name of this juvenile prodigy, so in case he is not provided with one, I would suggest his applying to his gracious sovereign for the title of Baron von—Munchausen.

HERBERT SADLER.

SATELLITES OF MARS.

Sir,—Is it fair and just to attribute the discovery of *both* the satellites of Mars to the American astronomer Prof. A. Hall? The outer satellite, we are told, has been seen at the Paris Observatory, *several times*, at Greenwich, and with a 7½-inch reflector of Cooke's. I am also informed that Borelly had seen them *both* before the announcement of their discovery in America.

How is it too, if they have been seen so long and so often, that they are not mentioned in any work on astronomy—Chambers, Herschel, Hind, &c., nor in the *Nautical Almanac?*

A SUBSCRIBER.

Oct. 1, 1877.

THE ROTATION OF SATURN.

In a recent number of the *Astronomische Nachrichten*, Prof. Asaph Hall, of the Naval Observatory, Washington, U.S., has published a determination of the time of rotation of Saturn, derived from observations of a bright white spot on the ball of the planet made at different stations in the United States.

The spot was first noticed by Prof. Hall on the night of, 1876, Dec. 7, and the time was observed of its transit over the line bisecting the disc, and perpendicular to the major axis of the ring; the observations were continued at Washington until, 1877, Jan. 2, and the spot was also observed in a similar manner at Cambridgeport, Hartford, and Poughkeepsie. The final result of these observations gives for the time of rotation of Saturn (on the supposition that the spot had no proper motion on the surface of the planet) 10h. 14m. 23·8h. ±2·3s. expressed in mean time.

In connection with this subject Prof. Hall has brought to light rather a curious circumstance. An ephemeris of this spot was sent off to different observatories throughout the country, computed by assuming the period of rotation to be 10h. 29m. 16·8s., but this value proved to be erroneous, and on account of the error several observers failed to see the spot. Prof. Hall states that this period of rotation is that given in nearly all modern text books, and is asserted by some writers to be Sir W. Herschel's final determination. But it seems that Sir W. Herschel never gave this value at all! The only published determination by that distinguished astronomer, as far as I know, is to be found in the Philosophical Transactions for the year 1794, p. 48, and the time of rotation there given is 10h. 16m. 0·4s., and it is difficult to say where the writers of text-books found the erroneous value. Prof. Hall suggests that it may have been taken from Laplace, who, in his "Système du Mond," says that the ring rotates in 0·437 of a day, which reduced to hours, minutes and seconds gives 10h. 29m. 16·8s., and someone taking this to be the time of rotation of the planet, succeeding writers have faithfully copied the mistake.

However, Prof. Hall is happy to be able to mention one American writer who gives Herschel's value *nearly* correct, viz., 10h. 16m. 4s.

Of English text-books that I have consulted on the subject, I find the value to be correctly given in Hind's "Solar System," and in Webb's "Celestial Objects for Common Telescopes" (3rd edition), it is also given correctly in the first edition of Chambers' "Descriptive Astronomy," but in the third edition the author adds that Herschel's latest determination was 10h. 29m. 16·8s., thus falling into the common error, whilst Mr. Proctor, who in the text of "Saturn and his System" gives the obnoxious value, adds in a foot note that an earlier determination gave the period of rotation to be 10h. 16m. 0·4s.

However this mistake may have arisen, it shows the danger of trusting to text-books for anything connected with the practical work and the advisability of consulting original publications whenever it is possible.

A. W. DOWNING.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN NOVEMBER, 1877.

By W. R. Birt, F.R.A.S., F.M.S.

In pursuance of our object in soliciting attention to Schröter's great valley, J. J. Cassini, we take this month its southern part in the lunar area II. I. τ. It will be remembered that the object last mentioned in our list for October, was "a broad plateau north of Fontenelle," the parallel of 65° N. latitude passes through this plateau, and in consequence its northern part is registered as II. I. τ 7.

Near this plateau are three conspicuous craters, shewn on B. and M.'s map A, *e* and *f*, A is Fontenelle, B of Schröter, which Neison places in 65° 32′ N. latitude, and 15° 39′ E. longitude; he describes it as moderately distinct, though not particularly bright; its symbol is II. I. τ 1, The crater *e* is registered as II. I. τ 3; it is placed by B. and M. on the floor of J. J. Cassini, but Schröter places his Z, which appears to be the same, on the west border of J. J. Cassini; *f* is in the next area to the east II. I. υ, its symbol is II. I. υ 1, it does not occur on Schröter's drawing.

This area, II. I. τ, takes in the middle part of the west border J. J. Cassini II. I. τ 5, the west portion of the middle of the floor II. I. τ 4, a steep peak II. I. τ 6, rising to the heights of 5,333 feet on the east part of the plateau II. I. ξ 3, II. I. τ 7, and also a point of considerable height II. I. τ 2, on the west border.

In comparing the objects specified in areas II. I. ξ and II. I. τ, which are north and south of each other, with Schröter's drawing, we must begin with *Fontenelle* and keep to the west border of J. J. Cassini as far as the high mountain ι (Isola) of Schröter, Anaxagoras γ of B. and M., we shall thus be able to verify Schröter's objects and compare their synonyms as given by B. and M., and reproduced by Dr. Schafarik on pp. 264, 265 of No. 178, October, 1877.

Buckhurst Hill: October 9, 1877.

SELENOGRAPHY.

It is proposed to establish a Club for the cultivation of Selenographical research. Gentlemen interested in the subject may communicate with Mr. Birt, of Buckhurst Hill, Essex, who will forward all particulars.

MINOR PLANETS (141), (174).

From *Astronomische Nachrichten*, No. 2160.

On the most recently discovered planets.—The planet the discovery of which by M. Borelly and by Mr. Watson was mentioned in No. 2148 of the *A. N.*, proves to be, according to the investigation of Dr. Knorre, identical with the planet (141) Lumen (see the Berlin *Planetary Circular*, No. 76). The planet discovered by Watson, on the 3rd of September, bears, therefore, the number (174).

October 10, 1877.

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF MARS.

Greenwich Noon.	Areographical longitude		latitude of the centre of ♂'s disc.	Angle of position of south pole.	Diameter of disc.	Greatest defect of illumination. Amount.	Position.
1877.	°	°	°	°	″	″	°
Nov. 1	16·6	350·6	26·45	160·95	15·52	1·45	65·9
2	7·2	·5	26·53	160·74	15·35	1·46	65·9
3	357·7	·5	26·61	160·54	15·18	1·47	66·0
—							
4	348·2	350·5	—26·69	160·34	15·02	1·47	66·0
5	338·7	·5	26·77	160·13	14·86	1·48	66·1
6	329·2	·5	26·84	159 92	14·70	1·48	66·1
7	319·7	·4	26·92	159 70	14·54	1·48	66 1
8	310·1	·5	26·99	159·47	14·38	1·48	66·2
9	300·6	·5	27·06	159·24	14·22	1·48	66·2
10	291·1	·4	27·12	159·01	14·07	1·48	66·2
—							
11	281·5	350·5	—27·19	158·77	13·92	1·48	66·2
12	272·0	·4	27·25	158·53	13·77	1·48	66·3
13	262·4	·4	27·31	158·29	13·62	1·48	66·3
14	252·8	·4	27·37	158·04	13·48	1·48	66·3
15	243·2	·4	27·43	157·79	13·34	1·47	66·3
16	233·6	·4	27·49	157·54	13·20	1·47	66·3
17	224·0	·4	27·54	157·28	13·06	1·47	66·3
—							
18	214·4	350·4	—27·59	157·02	12·92	1·47	66·3
19	204·8	·4	27·64	156·76	12·79	1·46	66·3
20	195·2	·4	27·68	156·49	12·66	1·46	66·4
21	185·6	·3	27·72	156·22	12·53	1·45	66·4
22	175·9	·4	27·76	155·95	12·40	1·45	66·4
23	166·3	·3	27·80	155·68	12·27	1·44	66·4
24	156·6	·4	27·84	155·40	12·14	1·43	66·4
—							
25	147·0	350·3	—27·87	155·13	12·02	1·43	66·4
26	137·3	·4	27·89	154·85	11·90	1·42	66·4
27	127·7	·3	27·92	154·57	11·78	1·41	66·4
28	118·0	·3	27·94	154·29	11·66	1·40	66 4
29	108·3	·3	27·96	154·01	11·54	1·40	66·4
30	98·6	·3	27·98	153·73	11·43	1·39	66·4
Dec. 1	88·9		27·99	153·45	11·32	1·38	66·4

A. M

Erratum.—Page 268, line 16, for 1787 read 1877.

ASTRONOMICAL OCCURRENCES FOR NOVEMBER, 1877.

DATE.			Principal Occurrences.	Jupiter's Satellites.		Meridian Passage
		h. m.			h. m. s.	h. m. Mars.
Thur	1		Sidereal Time at Mean Noon, 14h. 43m. 30·70s.			8 16·1
Fri	2		Sun's Meridian Passage 16m. 20·50s. before Mean Noon			8 13·2
Sat	3	16	Conjunction of Saturn and Mars 0° 11′ S.			8 10·4
Sun	4	20 47 15	● New Moon Conjunction of Moon and Mercury 5° 51′ N.			8 7·6
Mon	5					8 4·9
Tues	6		Saturn's Ring: Major axis=41″·19 Minor axis=2″·81	3rd Oc. D.	5 57	8 2·1
Wed	7			1st Tr. I. 1st Sh. I.	5 49 6 46	7 59·4
Thur	8	4 26 5 37 11 13	Occultation of B.A.C. 6072 (6½) Reappearance of ditto Conjunction of Moon and Venus 1° 50′ N. Conjunction of Moon and Jupiter 4° 7′ N.	1st Ec. R.	6 14 47	7 56·8
Fri	9	16	Conjunction of Jupiter and Venus 2° 42′ S.			7 54·1
Sat	10					7 51·5
Sun	11					7 48·9
Mon	12	11 44	☽ Moon's First Quarter			7 46·4
Tues	13	5	Superior conjunction of Mercury and Sun			7 43·8
Wed	14	13 22	Conjunction of Moon and Saturn, 4° 0′ S. Conjunction of Moon and Mars 3° 34′ S.			7 41·3
Thur	15	12 55 13 55	Occultation of 21 Piscium (6) Reappearance of ditto Illuminated portion of disc of Venus=0·618 Illuminated portion of disc of Mars=0·890	2nd Oc. D. 1st Oc. D.	5 3 5 5	7 38·8
Fri	16	13 25 13 37	Occultation of 51 Piscium (6) Reappearance of ditto	1st Tr. E. 1st Sh. E.	4 38 5 28	7 36·4
Sat	17		Sidereal, Time at Mean Noon 15h. 46m. 35·61s.	2nd Sh. E.	4 10	7 33·9
Sun	18		Sun's Meridian Passage 14m. 37·45s. before Mean Noon			7 31·5

DATE.			Principal Occurrences.	Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m.
Mon	19	3 57	Occultation of μ Arietis (5½)			7 29·1
		4 49	Reappearance of ditto			
		13 39	Near approach of ϵ Arietis (4½)			
Tues	20	10 19	○ Full Moon			7 26·7
		7 19	Occultation of 16 Tauri (5½)			
		8 24	Reappearance of ditto			
		7 22	Occultation of 17 Tauri (4)			
		8 10	Reappearance of ditto			
		7 42	Occultation of 19 Tauri (5)			
		8 38	Reappearance of ditto			
		7 49	Occultation of 20 Tauri (5)			
		8 55	Reappearance of ditto			
		9 2	Near approach of η Tauri (3)			
		21	Conjunction of Venus and ψ Sagittarii 0° 4′ S.			
Wed	21	21	Uranus at quadrature with the Sun			7 24·4
Thur	22	11 17	Near approach of 136 Tauri (5)			7 22·0
Fri	23	12 8	Occultation of 39 Geminorum (6½)			7 19·7
		13 7	Reappearance of ditto	1st Tr. I.	4 22	
		12 23	Occultation of 40 Geminorum (6½)	1st Sh. I.	5 5	
		13 37	Reappearance of ditto			
Sat	24	15 12	Near approach of γ Cancri (6½)	1st Ec. R. 3rd Tr. I.	4 32 50 4 57	7 17·4
		16 5	Near approach of μ^1 Cancri (6)	2nd Tr. E.	5 18	
Sun	25					7 15·1
Mon	26		Saturn's Ring: Major axis=39″·83 Minor axis=2″·68			7 12·9
Tues	27	10 5	☾ Moon's Last Quarter			7 10·6
		13 26	Occultation of 37 Sextantis (6)			
		14 14	Reappearance of ditto			
Wed	28	14 33	Near approach of υ Leonis (4½)			7 8·4
Thur	29			4th Sh. E.	4 33	7 6·2
Fri	30					7 4·0
DEC. Sat	1			2nd Tr. I.	5 15	7 1·8

THE PLANETS FOR NOVEMBER.

At Transit over the Meridian of Greenwich.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	14 5 30	S. 11 35½	4″·8	23 18·2
	9th	14 55 30	S. 16 32½	4″·6	23 36·6
	17th	15 40 7	S. 20 10½	4″·6	23 56·1
	25th	16 32 40	S. 23 20	4″·8	0 14·3
Venus ...	1st	17 28 23	S. 25 45½	17″·2	2 44·4
	9th	18 9 18	S. 26 9½	18″·4	2 53·8
	17th	18 49 36	S. 25 48	19″·6	3 2·5
	25th	19 28 35	S. 24 44	21″·0	3 9·9
Mars ...	1st	23 0 57	S. 8 39	18″·4	8 16·1
	9th	23 10 28	S. 7 30	16″·8	7 54·1
	17th	23 21 46	S. 5 31	15″·5	7 33·9
	25th	23 34 30	S. 3 46	14″·3	7 15·1
Saturn ...	1st	23 3 45	S. 8 24	16″·6	8 18·9
	9th	23 3 9	S. 8 26½	16″·5	7 46·8
	17th	23 2 57	S. 8 26	16″·3	7 15·2
	25th	23 3 11	S. 8 23	16″·0	6 43·9
Neptune ...	1st	2 17 24	N. 11 48½	...	11 32·0
	17th	2 15 43	N. 11 40	...	10 27·4

Mercury is badly situated for observation, rising on the 1st about 40 minutes before the sun, the interval rapidly decreasing. After the middle of the month he sets after the sun.

Venus sets nearly two hours after the sun at the beginning of the month, the interval increasing.

Mars may be observed till one hour and a half after midnight on the 1st, the interval decreasing.

Saturn is visible till one hour 40 minutes after midnight at the beginning of the month, the interval decreasing. He sets a little before midnight on the last day.

THE FIVE INNER SATELLITES OF SATURN.

Longitudes of the five inner satellites reckoned from upper conjunction, and expressed in decimal fractions of a revolution.

8h.Gr. Nov.	Mi.	En.	Te.	Di.	Rh.
1	·875	·948	·127	·966	·465
2	·936	·678	·657	·332	·687
3	·997	·408	·186	·697	·908
4	·058	·138	·716	·062	·129
5	·119	·868	·246	·428	·351
6	·180	·598	·775	·793	·572
7	·241	·327	·305	·159	·794
8	·302	·057	·835	·524	·015
9	·363	·787	·365	·889	·236
10	·424	·517	·894	.255	·458
11	·485	·247	·424	·620	·679
12	·546	·976	·954	·985	·901
13	·607	·706	·483	·351	·122
14	·668	·436	·013	·716	·343
15	·729	·165	·543	·081	·565
16	·790	·895	·072	·447	·786
17	·851	·625	·602	·812	·007
18	·912	·355	·132	·178	·229
19	·973	·085	·661	·543	·450
20	·034	·814	·191	·908	·671
21	·094	·544	·721	·274	·893
22	·155	·274	·250	·639	.114
23	·216	·003	·780	·004	·335
24	·277	·733	·310	·370	·556
25	·338	·463	·839	·735	·778
26	·399	·193	·369	·100	·999
27	·460	·922	·898	·465	·220
28	·521	·652	·428	·831	·442
29	·582	·382	·958	·196	·663
30	·643	·111	·487	·561	·884

To find the longitudes for other hours and, by their means, the distances of the satellites from the minor axis of the ring, v. the tables p. 278.

Approximate Greenwich mean time of some of the conjunctions of the satellites with the ends of the ring, v. p. 279.

1877.	Gr. M. Time. h.		
Nov. 1	5·2	Mimas.	w.
	5·8	Tethys.	nf.
	6·2	Dione.	np.
	6·4	Encel.	np.
	7·1	Rhea.	sf.
	8·6	Dione.	npl.
	9·9	Rhea.	nfl.
2	5·4	Encel.	sp.
	10·4	Encel.	w.
3	7·7	Encel.	sf.
4	6·7	Encel.	nf.
	7·9	Dione.	nf.
5	8.8	Dione.	sf.
	9·1	Encel.	np.
	11·0	Mimas.	e.
	11·1	Dione.	sfl.
6	8·1	Encel.	sp.
	9·6	Mimas.	e.
7	5·4	Encel.	e.
	5·5	Tethys.	e.
	8·2	Mimas.	e.
	10·4	Encel.	sf.
8	6·8	Mimas.	e.
	8·1	Dione.	spl.
	9·4	Encel.	nf.
	10·4	Dione.	sp.
	11·0	Rhea.	nf.
	11·9	Tethys.	np.
9	5·4	Mimas.	e.
	6·8	Encel.	w.
	10·9	Tethys.	sf.
	11·3	Dione.	np.
10	8·0	Rhea.	sf.
	9·2	Tethys.	np.
	10·7	Rhea.	sfl.
	10·7	Encel.	sp.
11	7·9	Tethys.	sf.
	8·1	Encel.	e.
	10·0	Tethys.	sfl.
12	5·0	Dione.	np.
	5·5	Encel.	np.
	6·5	Tethys.	np.
	7·3	Dione.	npl.
	8·6	Tethys.	npl.
13	5·2	Tethys.	sf.
	7·3	Tethys.	sfl.
	9·4	Encel.	w.
	10·2	Tethys.	spl.
	12·3	Tethys.	sp.
15	6·6	Dione.	nf.
	7·5	Tethys.	spl.
	9·6	Tethys.	sp.
	10·8	Encel.	e.
16	7·1	Mimas.	w.
	7·5	Dione.	sf.
	8·1	Encel.	np.
	8·3	Tethys.	nf.
	9·8	Dione.	sfl.
17	5·7	Mimas.	w.
	6·9	Tethys.	sp.
	7·2	Encel.	sp.
	11·8	Rhea.	nf.
18	5·6	Tethys.	nf.
	9·5	Encel.	sf.
19	6·8	Dione.	spl.
	8·5	Encel.	nf.
	8·8	Rhea.	sf.
	9·1	Dione.	sp.
20	5·9	Encel.	w.
	10·1	Dione.	np.
21	9·8	Encel.	sp.
22	7·2	Encel.	e.
	10·1	Mimas.	e.
23	8·8	Mimas.	e.
	11·2	Encel.	nf.
	11·7	Dione.	nf.
24	6·5	Rhea.	sp.
	7·4	Mimas.	e.
	8·5	Encel.	w.
25	5·9	Encel.	sf.
	6·0	Mimas.	e.
	11·7	Tethys.	np.
26	4·9	Encel.	nf.
	5·4	Dione.	nf.
	9·9	Encel.	e.
	10·4	Tethys.	sf.
27	6·3	Dione.	sf.
	7·3	Encel.	np.
	8·6	Dione.	spl.
	9·0	Tethys.	np.
	11·1	Tethys.	npl.
28	6·3	Encel.	sp.
	7·7	Tethys.	sf.
	9·7	Rhea.	sf.
	9·8	Tethys.	sfl.
29	6·4	Tethys.	np.
	8·6	Encel.	sf.
30	5·0	Tethys.	sf.
	7·1	Tethys.	sfl.
	7·6	Encel.	nf.
	7·9	Dione.	sp.

On Nov. 7 and 23 the shadow of *Titan* will pass across the disc of Saturn. By watching on these evenings observers may secure most

valuable results, by fixing the precise time when the shadow crosses the central meridian, and the point where it crosses. On Nov. 6 *Iapetus* will be near its greatest eastern elongation, and on Nov. 25 near its inferior conjunction, north.

A. M.

NEW COMETS.

A new comet was discovered at Florence at the Observatory of Arcetri, by Prof. Tempel, on the 2nd October, at 8 o'clock, near the star *Iota* of Cetus. It had a nucleus like a star of the 11th magnitude, and a tail fan-shaped 5 minutes of arc in length. It is moving rapidly towards the south, and will consequently be only a short time visible in Europe. Its position on October 2nd, at 9h. 25m. (M. T. Arcetri) was R. A. 357° 45′, Decl. —10° 19′. This is the fifth comet of the present year.—*La Nazione*, October 4th.

Elements of Comets, 1877, Sept. 14, 18, Oct. 6, with Ephemeris by E. Hartwig.

T. 1877, September 10·7566. Berlin M.T.

	°	′	″
π – ☊	142	52	55
☊	251	3	52
i	102	8	54

lg. q. 0·197506.

Mittl.	Beob.	(R—B)
d λ	cos. s.	+15″
	a s	—6″

Ephemeris for 12h. M.T. Berlin.

d	*a* h. m. s.	*δ* ° ′	log. Δ	log. *r*
Oct. 7	8 6 20	+41 9·6	0·1606	0·2089
11	7 58 47	39 36·5	0·1385	0·2124
15	7 49 59	37 53·1	0·1153	0·2163
19	7 39 49	35 56·8	0·0912	0·2207
23	7 28 8	33 44·4	0·0667	0·2253
27	7 14 53	31 12·4	0·0423	0·2303
31	7 0 0	28 17·7	0·0188	0·2356
Nov. 4	6 43 31	24 57·4	2·9973	0·2412
8	6 25 37	21 10·9	9·9791	0·2470

From *Astronomische Nachrichten*, No. 2160.

(Telegram from the Imperial Academy of Sciences at Vienna.)

Comet Tempel, 2 October 9h. 24m., Florence; *a* = 357° 45′, P = 100° 19′; —70′, +63′; small, bright, tail.

1877, October 3.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Sept., 1877.

Lean, W. S.
Lee, G. H.

To Oct., 1877.

Beechey, Rev. Canon

To Dec., 1877.

Baron, Rev. J.
Cox, Rev. H.
Potter, Rev. T. J.
Squire, H.

To Mar., 1878.

Rivas, Miss

To Dec., 1878.

Birt, W. R.

To June, 1879.

Burnham, S. W.

The Astronomical Register is intended to appear at the commencement of each month; the Subscription (including Postage to all parts of Great Britain and Ireland) is fixed at **Three Shillings** per Quarter, ***payable in advance***, by postage stamps or otherwise.

The pages of the *Astronomical Register* are open to all suitable communications. Letters, Articles for insertion, &c., must be sent to the Rev. J. C. Jackson, *Clarence Road, Clapton, E.*, **not later than the 15th of the Month.**

The Astronomical Register.

No. 180. DECEMBER. 1877.

ROYAL ASTRONOMICAL SOCIETY.

Session 1877—78.

The First Meeting after the long vacation, Friday, November the 9th, 1877.

William Huggins, Esq., D.C.L., LL.D., &c., *President*, in the Chair.

Secretaries—Mr. Glaisher and Mr. Ranyard

The minutes of the preceding meeting were read and confirmed.

The following gentlemen were elected Fellows of the Society:

Robert C. Carrington, Esq., Indian Marine Survey, Calcutta.

Alexander Davidson, Esq., Addison Lodge, Ridgway, Wimbledon.

Thomas Gregory, Esq., Merchant's College, Blackpool.

Lieut. Frederick William Jarrad, R.N., Deputy Superintendent, Indian Marine Survey.

A. K. Rollit, Esq., LL.B., B.A., Sheriff of Hull, President of the Royal Institution, Hull, 18, Trinity House Lane, Hull.

Mr. Glaisher announced that 131 presents had been received by the Society since the last meeting, amongst which were some drawings of Mars, which had been made by Mr. Green during the past summer in the Island of Madeira. He also wished to notice the new supplementary catalogue of scientific papers which had been published by the Royal Society. The volume upon the table contained the names in the first part of the alphabet, from A to H, and the period included was from 1864 to 1873.

The President said: The Society will rejoice to hear that there lie upon the table duplicate copies of the very successful observations of Mars, which Mr. Gill has succeeded in making at the Island of Ascension. I think the Society will be the more gratified when they remember the very great difficulties which Mr. Gill has had to encounter. I think I may venture to say that these successful observations would not have been obtained had it not been for his remarkable decision of character. When he found that the site that had been selected for his observatory, and where the instruments had been fitted up, was unsuitable, and that the sky would probably be cloudy, he at once resolved to cross over to an uninhabited part of the island, and there in the face of very grave and almost insurmountable difficulty, he proceeded to mount his instruments. This decision of his has been, I believe, the means of our obtaining successful observations. I will now call on the Foreign Secretary to read some portion of Mr. Gill's report.

Lord Lindsay said: I do not think it will be necessary to take up the time of the meeting in reading the report in full, and I will endeavour to give a *précis* of it. Mr. Gill gives his reasons for dismounting the observatory at the garrison and removing it to its present site. These reasons are that the Green Mountain at Ascension brings down low clouds of moisture upon the town, and he felt convinced that the chances of making observations would be lost. He therefore crossed to another part of the island, and landed at a place hitherto unknown, but now called Mars Bay, and here he proceeded to set up his observatory. A considerable number of measurements have been secured here, which no doubt could not have been obtained in the garrison. The labour of watching had been excessive. Mars had been observed since the 10th of August, whenever visible, except on three occasions, when the definition was unsatisfactory. These occasions were in the evening. His report goes on to give a description of the site he had chosen, and includes some photographs of the observatory as now set up. On 32 evenings and 25 mornings he had obtained measures of the distance of the limb of the planet, from 22 different stars of comparison. The rain prevented observations between Sept. 11th and 15th, but between Sept. 4th and 11th the weather was exceptionally fine. To the latter-named period Mr. Gill attached extreme importance, because Sept. 5th was the day of opposition, when the planet would pass within two minutes of arc of an important comparison star. Mr. Gill says he would have willingly undertaken the expedition merely for some of the results obtained, and he records his thanks to the Society for their liberality in aiding the expedition. A description of the observations and tables sent then follows in the report, as well as an enumeration of the observatories that have promised assistance in determining the place of the comparison stars. After spending a week up the mountains, Mr. Gill says he intends to devote some time to the observations of Melpomene.

Lord Lindsay concluded by saying that private letters from Mr. Gill informed him that the change of air up the Green Mountain had quite restored his health.

The Astronomer-Royal: I have been in correspondence with Mr. Gill on this subject, and possibly I have received some information which does not appear so entirely in this official report, which has been read by Lord Lindsay. I think that when the history of this expedition comes to be written, it will be considered as one of the most important and most remarkable that has ever been undertaken. There was one point naturally not touched upon in the official communication made by Lord Lindsay; it is the assistance which Mr. Gill has received from another person. We are not ashamed, in speaking of the efforts which Sir William Herschel made in his important stellar observations, to speak of the assistance which he received from his sister, Miss Caroline Herschel, and I hope we shall not be ashamed in this case to speak of the assistance which Mr. Gill has received from his wife. (Hear, hear.) From all that I have gathered from his communications, I believe that the expedition, under all its difficulties, could not have been carried out without the assistance of that lady, and I should be sorry that on this occasion such a notice should be omitted.

The President: I can quite corroborate the statement of the Astronomer-Royal from letters I have received, and also the statement of Lord Lindsay that Mr. Gill has found his health completely restored by his visit to the mountain regions. Looking back at the period of work, Mr. Gill says that the whole may be summed up in three words, "watching, anxiety, and some good results." He is now working at Melpomene. While observing Mars he could not spare time to observe the smaller planets, but now he is devoting his time to it.

The thanks of the meeting were voted to Mr. Gill for his report.

The Astronomer-Royal was then called upon. He said, it is sufficiently well known to the Society that I have been looked upon by the Government as their representative in late expeditions for the observations of the transit of Venus. The paper I propose now to read in great measure has reference to those observations. It is entitled *On the inferences for the value of the mean solar parallax and other elements, deducible from the telescopic observations of the transit of Venus*, 1874, *Dec.* 8; *made in the British Expedition.* The Society are aware that a report to the House of Commons on the observations has recently been printed, and is now on sale. Although all the important points are fully treated in the report, several minor matters have been necessarily deferred, some of which did not present themselves for consideration, and some of which, under pressure of time, it was difficult to examine. I propose in the present paper to supplement the Parliamentary paper, and to describe the process which has been pursued in the reductions. At some future time I propose to treat these observations and the reductions, with any variations which time may suggest, in a more orderly form, and these with the additional inferences to be derived from the photographic observations will, I hope, be printed under the auspices of the British Government.

Acting as official-superintendent, and under official responsibility, I have examined and proved every part of the operations, but I am bound to state that the most important work in discriminating the classes of observations, in the scrutiny of individual observations, in the organization of an efficient computing staff, and in the general maintenance of order in the calculations is due to Captain Tupman. I mean to say I am acquainted with every figure that has been given in the report, but I am very much indebted indeed to Captain Tupman. The conclusion which we have brought out is that the mean distance of the sun's centre is 93,375,000 miles. Considering that the number of observers was eighteen, and that they made fifty-four observations, and considering also the degree of training they have had, and their zeal, and the extreme care that was taken in the choice of stations, I think that there will not be anything to compete with the value which has been deduced. I could have wished, however, that we had been able to secure egress observations in New Zealand, and more successful egress observations in Kerguelen; but, however, we have done extremely well without them. I think that I may say that the results I now present to the Society are well worthy of very great confidence. We are at present engaged at the observatory on the photographic observations, but I doubt whether the results obtained from them will interfere with those which have been deduced from the telescopic observations.

Captain Tupman (being called on by the President) said: I do not think I have anything to add upon the subject. My work has all been dictated to me by the Astronomer-Royal, and I have only been interested in carrying out his instructions. At the same time, however, I may say that I have had a good deal to do with reading the reports of the various observers, and have spent months in studying the expressions they use and in putting two and two together, so as to make out exactly what phase they refer to, and to satisfy myself that observers, who were three or four thousand miles apart, were referring to the same thing. When it had once been firmly impressed upon my mind that they did mean the same thing, then I consider that the equations formed from their observations were free from serious errors. There would of course be errors in the longitude and consequently in the time, but I may say that we have nearly all the longitudes free from exceeding errors, except a second or two of time; but what I wish particularly to say is, that I think the final determination of the distance of the sun, that has been deduced from all these observations, is the most satisfactory yet obtained, and is the most free from systematic errors. It is, I should say, within 200,000 miles of the truth. (Hear, hear.)

Professor Adams was then called upon to read a paper *On the motion of the moon's node in the case when the orbits of the sun and moon are supposed to have no eccentricities, and their mutual inclination is supposed to be indefinitely small.*

The author began by calling attention to an extremely able paper by Mr. G. W. Hill, assistant in the American "Nautical Almanac" Office, on the part of the motion of the lunar perigee, which is a function of the mean motion of the sun and moon. He then stated that several years ago he applied a method similar to Mr. Hill's to the determination of the motion of the moon's node, supposing the orbit to have no independent eccentricity.

The motion of the perigee in one case, and that of the node in the other, are found by means of the evaluation of an infinite determinant. The result which the author obtained is the following:

If m denote as usual the ratio of the sun's mean motion to that of the moon, and g be the ratio of the mean motion of the moon from the node to its mean sidereal motion, and if we take $m = 0\cdot0748013$, then in the ideal case considered $g = 1\cdot00399{,}91618{,}466$. The value of the same quantity found from Delaunay's development in powers of m, which is carried as far as m^9, is $g = 1\cdot00399{,}91723$. The author has found that the series for g converges more rapidly when developed in powers of $\frac{m}{1-m}$. The value of g found from the same number of terms of this series is $g = 1\cdot00399{,}91591$, which is seen to be a much closer approximation than the former.

The Astronomer-Royal: I understand the movements of the node obtained in this way are when the inclination is indefinitely small?

Professor Adams: Yes, or in other words, when powers of the inclination above the first are neglected.

The Astronomer-Royal: And when there is no eccentricity in the ordinary sense of the word?

Professor Adams: Quite so.

The Astronomer-Royal: And, in the third place, I understand the Variation is to be taken into account?

Professor Adams: Yes; that is, the inequalities independent of the eccentricities are fully taken into account.

The Astronomer-Royal: I mean the variation in longitude.

Professor Adams: Yes.

Mr. Glaisher: Is Delaunay more accurate or less accurate than might be expected?

Professor Adams: The degree of approximation is just what might have been expected.

Professor Cayley: This is a very remarkable analytical process which Mr. Hill has used in a physical investigation. It is, so far as I am aware, the first instance in which an infinite determinant has presented itself. I mean a determinant with an infinite number of rows, and an infinite number of columns.

The Astronomer-Royal: Perhaps I may make one remark. Be it far from me to criticise anything coming from Professor Adams; in point of fact what I have to say does not at all apply to the special subject of this communication, but to an object collateral with it. The object of Professor Adams in the investigations he has explained was to find the motion of the moon's node under very limited conditions. Now it is known to members of this Society that I have been long engaged upon an investigation of the lunar theory, which goes upon a totally different supposition, namely, that all the elements have considerable magnitudes, that is, all the elements which are set aside here have considerable magnitudes, which must be taken into account if anything like a complete development of the lunar theory is wanted, which will present the moon to us as seen in the skies. The admirable development which Professor Adams has explained now is an imaginary one. I do not wish to cast any slur upon it by the use of the word "imaginary," but it is one which does not apply to a real moon. I have endeavoured by a cumbrous and awkward process to do something towards a theory for the real moon. In this way it is possible. If a given equation was of the fourth order, for instance, for which no formula of solution exists, it is possible to give a solution of it, or a very proximate solution, and you may carry on the solution to any degree of accuracy you desire, provided you abandon your analysis, and take to it in a lower or more practical method, treating the equation with numbers. I merely mention this at the present time to show how highly I esteem the analytical process which Professor Adams has explained, and has carried further by this most elegantly imaginary theory.

The President: I am sure the use of the adjective "imaginary" will not diminish the thanks of the Society to Professor Adams (Hear, hear).

The President then called upon Mr. Neison to read a paper entitled *On a new term of long period in the mean motion of the moon.* A few years back Sir G. Airy, after showing that there existed grave discordancies between the observed place of the moon and the place assigned by the best theoretical tables, declared that he believed a serious defect to exist in the lunar theory. Mr. Neison now announced that he believed he had discovered a new term of long period which would go far to remove these discordancies. The new term has an argument which depends on the mean motion of the moon, minus 24 times the mean motion of the earth, and plus 20 times the mean motion of Mars. Its maximum value amounts to rather more than seven and a half seconds of arc, and its period is over 400 years. It was stated that this new term of long period satisfactorily accounted for all the discordancies between the theoretical and observed places of the moon since 1775, leaving outstanding errors of generally under one second of arc. This new term is perfectly analogous in form to the well-known term discovered by Professor Hansen in 1847, and both are due to the direct action of the disturbing planet, and are also both of the third order of the eccentricities. The new term derives its large value from the magnitude of the eccentricity of the orbit of Mars, which is nearly 14 times greater than that of the orbit of Venus.

Mr. Neison had also a second paper *On three small inequalities in the mean motion of the earth, and an inequality in the mean motion of Mars*, which referred to some new terms of small magnitude in the motions of the earth and Mars, similar to some which had been already discovered by Leverrier.

The President: We have had our attention directed to physical astronomy in the older sense of the word, we will now pass to physical astronomy in its more modern sense, and we will take together three papers on the physical character of Mars.

Mr. Green read a paper *On observations of Mars made during the months of August and September*, 1877, *in the island of Madeira.* There were 47 nights during which the telescope was ready for work, of which 26 were favourable for the pencil. Ten nights were classed as good, four excellent, and two as superb; the remaining 10 were passable, but poor. Out of the 47 nights there were thus 16 good ones. The instrument used was a 13 inch silver on glass reflector. The drawings made furnish evidence of the general clearness of the atmosphere of Mars; but the presence of an atmospheric envelope is abundantly shown by the decrease in the distinctness of the details towards the limb, and by an absence of marking towards the North Pole. He had given special attention to the points with regard to which queries had been put by Dr. Terby, and he was of opinion that some of the planetary details, which had perhaps been rather too precipitately named after modern observers, would require modification, and some would have to be excised altogether. In conclusion, he wished to express his thanks to Mr. Marth for the valuable hints which he had furnished as to the dates when certain markings would be on the meridian.

Captain Noble: I should like to ask Mr. Green whether he observed that the markings were much more distinct on the following limb than on the preceding limb, as though the sunrises were clearer on Mars than the sunsets?

Mr. Green: The only answer I can make is that I noticed that the markings about the pole were much better seen on the side towards sunset than sunrise.

Captain Noble: I am speaking of the dark markings shading off towards the limb. They appeared to me to shade off much more rapidly towards the preceding limb than towards the following limb. In one or two cases I traced details right up to the latter limb.

Mr. Green: I can answer that question more clearly by referring Captain Noble to the drawings.

The Astronomer-Royal: Would it not be better that questions of this kind should be put through the President?

The President: I was going to suggest that these points will come out perhaps better after the other papers on Mars have been read.

The Astronomer-Royal: I am glad to hear there is something hinted at like an irregularity of outline of the white polar spot of Mars. I am always glad to hear of instances of such irregularity; there is another one of a similar kind which has recently been pointed out; I mean the irregularity of outline which has been ascribed by Mr. A. Hall to the inner ring of Saturn. He thinks he has established that the inner ring has not a definitely clear elliptical outline, but that there are points sticking out from various parts of it.

Mr. Christie read a paper entitled *Physical observations of Mars, made at the Royal Observatory, Greenwich.* The spectrum of Mars had, he said, been examined with special reference to detecting any difference between the spectrum of different parts of the disc, and also with the object of comparing the absorption bands of Mars with those caused in our own atmosphere. One curious result at which we have arrived is that the absorption bands in the spectrum of the planet are most marked, not upon the limb, but just inside the limb, at a short distance from it.

Captain Noble said his paper had reference to the point concerning which he had put his question to Mr. Green, namely, that he had found that in the majority of cases the markings upon the planet were sharper and darker towards the following limb than the preceding one. The definition varied very considerably, but wherever he had succeeded in tracing the markings nearly up to the limb, it was always the following limb, and then indistinctness was invariably much greater on the preceding limb.

Mr. Brett read a paper *On the physical condition of Mars.* He had made a series of telescopic observations from August 2nd to October 8th with a 9 inch Browning reflector, near the Lizard signal station. The conditions were favourable but the results were unsatisfactory, inasmuch as they tended to disturb the conclusions which had hitherto been generally received. The modern drawings of Mars exhibited half-tints with dark spots near the equatorial regions; but his observations showed that the planet's atmosphere was sufficiently thick to prevent a telescope doing justice to details. The opacity of the atmosphere was shown by the great brightness of the disc at the limb. The theory that the redness of the so-called land was owing to the absorption of the planet's atmosphere was untenable. Both the snow and the seas must be given up. The conclusion of the speculative astronomer that Mars was a habitable globe must be given up, for there were no clouds, and as far as the so-called snow was concerned, it must lie considerably above the surface of the planet. His hypothesis to account for the white polar patch was that it consisted of clouds in the neighbourhood of the pole, which was the only region cold enough to permit of the condensation of vapour; the clouds must exist at the outer region of the atmosphere, for he had observed their shadow cast upon the planet. The sum of the results was disappointing, because the snows must go and the seas must go, and the little planet would have its character taken away.

Lord Lindsay* read a paper *On an arrangement of the spectroscope for the examination of nebulæ and objects of considerable angular diameter.* He said: as generally used in conjunction with the telescope the slit of the spectroscope is placed in the focus of the object-glass. When a star is observed it becomes necessary to use a cylindrical lens with its axis at right angles to the slit, in order to give the spectrum sufficient breadth to render the lines visible; if the cylindrical lense is dispensed with and the instrument directed to a nebula it is obvious that a spectrum will be seen of a height corresponding to the diameter of the nebula, and the spectrum will consist of a series of images of the small elementary part of the nebula, the light of which falls within the jaws of the slit. In many cases the light of these nebulæ are too feeble to permit of the fainter lines of the spectrum being seen. It would, consequently, be a great advantage if we could combine the whole of the light given by the nebula to form a spectrum of increased brilliancy; this may be partially done by removing the slit from the principal focus and placing it behind the eye-piece, at the place where the parallel pencils from all parts of the image cross—at this point we may be said to have an image of the object-glass which may be compressed into a narrow linear form, by interposing between the object-glass and the slit a cylindrical lens with its axis parallel to the length of the slit. To make use of such a combination a spectroscope with collimator and telescope of very short focal length will be necessary, so that the whole of the pencil falling within the jaws of the slit from all parts of the object may be utilised. Trials made with very imperfectly arranged portions of various spectroscopes show the spectrum of the great nebula in Orion sometimes as a bright continuous spectrum, resulting from the enclosed stars on which the nebula lines stand out with great brilliancy, but by selecting a part free from stars the spectrum is seen as a faint continuous spectrum with very bright lines, the fourth or G line being characterised by its violet colour. The nebula in Andromeda gave a bright continuous spectrum, not without a suspicion of bright lines. It may almost seem paradoxical, but the spectrum of this nebula appeared very much brighter than the nebula itself, seen in an ordinary eye-piece, but it will be seen that the spectrum is an integration spectrum, and not a spectrum of any particular part of the nebula falling within the jaws of the slit.

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* Owing to a mistake in directing a letter, proofs of the following paragraphs have not been submitted to Lord Lindsay for correction.

Lord Lindsay then drew attention to the variable star in Cygnus, in connection with the single bright line about which he had sent a communication to *The Times*. When he first saw it, on November 26 last year, the star was giving a bright line spectrum. It was not seen again at his observatory till the 2nd January, when good measures were taken of the bright lines, but press of work prevented it being observed again till September, when the spectrum of the star was not recognised at first, for it looked exactly the same as when looked at without the spectroscope; the light was almost entirely monochromatic. It had exactly the same appearance as looking at a small planetary nebulæ with an ordinary eye-piece. On looking at a number of small stars in its neighbourhood, and then putting a prism in front of the eye-piece, all the small stars disappeared except this one, which shone out with the same vigour as before. A telegram received that evening informed him that it was still in the same state.

The President: With regard to the use of a cylindrical lens in front of the slit, I had a similar apparatus constructed by Mr. Browning, 14 years ago. I did not find any advantage in its use on nebulæ and planets, it may be because I did not use a collimating lens of sufficiently short focus, but it is obvious that as the collimator is shortened the slit must be made narrower. I will now ask the meeting to proceed to the ballot.

The following papers were also laid upon the table:

T. H. Safford: *Note upon the proper motion of Groombridge* 3511

G. D. Hirst: *Remarks to accompany drawings of Mars.*

H. Pratt: *Total lunar eclipse, August* 23, 1877.

J. E. H. Peyton: *Note on the eclipse of the moon, August* 23, 1877.

Rev. S. J. Perry: *Phenomena of Jupiter's satellites,* 1875—77.

Rev. S. J. Perry: *Lunar eclipse, August* 23, 1877.

W. H. Bixby: *Eclipses of Jupiter's satellites in* 1875, *observed at Willet's Point, U.S.A.*

S. M. Drach: *On the synodisms of Saturn's satellites.*

J. M. Wilson and G. M. Seabroke: *Note on the second catalogue of micrometrical measures of double stars, made at the Temple Observatory, Rugby.*

Prof. C. V. Zenger: *On the relative brightness of the details of Jupiter's disc and satellites.*

E. Dunkin: *On the proper motions of Groombridge* 2275 *and* 2276 (Σ 1972).

E. Dunkin: *Note on the supposed physical association of Groombridge* 3477 *and* 3511.

E. Crossley and J. Gledhill: *A working list of binary and other interesting double stars.*

V. Fasel: *Observations of the lunar eclipse, August* 23, 1877, *made at Morges, Switzerland.*

Isaac Ward: *Observations of the satellite of Neptune.*

H. C. Russell: *On the atmospheric lines between* D_1 *and* D_2 *in the spectrum.*

The Astronomer-Royal: *On the spectrum of a solar spot observed at the Royal Observatory, Greenwich.*

Prof. C. Pritchard: *Elements and ephemeris of Coggia's comet.*

J. W. L. Glaisher: *On the law of force tending to any point whatever in the plane of motion, in order that the orbit may always be a conic.*

The meeting adjourned at ten minutes past 10.

REVIEWS.

[The following have been unavoidably postponed by stress of matter.]

Les Merveilles Célestes, Lectures du Soir, par Camille Flammarion, Auteur de la Pluralité des Mondes habités. Cinquième édition, illustrée de 84 vignettes astronomiques et de trois cartes célestes. Paris, Librairie Hachette et Cie. 1875. [2fr. 25c.]

In the preface to this fifth edition the author says,- "Twenty-five thousand copies of this little book have gone to scatter the good seed a little everywhere, and lift before the most varied minds a corner of the veil that even in the present day hides from almost all eyes the sublime and divine spectacle of nature. By the perusal of this altogether elementary exposition, it is possible in fact at once to begin to see, to admire, to comprehend the general structure of the universe, in the midst of which the earth is but an atom. And how many live and die without any suspicion of the truth! It has been our aim here not only to teach, but principally to diffuse a taste for the study, and to show how pleasant it should be to be instructed. We ask of our young readers that they would allow their understandings merely to approach the border of the panorama revealed by science; they will not be slow to imagine that the purest pleasures of our life consist in the contemplation of nature; and very soon their fervid ardour will eagerly desire to comprehend the great beauties of creation."

M. Flammarion has done much to disseminate a knowledge of astronomy in France, and the brisk circulation of this little volume shows that his endeavours are appreciated. It is written in a lively and attractive style, interspersed with quotations from English, French, and German poets, and copiously illustrated. The author desires to keep it up to the level of the rapid progress of discoveries, both as regards the text and the illustrations. As to the latter, the first edition, in 1865, contained 30 drawings, whilst the present has 87. On pp. 296, &c., the effects of the cessation of the earth's movements of rotation and revolution is considered—though such a thing is pronounced impossible. It might, however, have been well to advert to the, however remote, effect of ethereal friction by which the earth and planets, it is thought, would be finally precipitated on the sun; and to the gradual dissipation of the earth's energy of rotation. Again; the once prevailing opinion of the excessive thinness of the crust of the earth—not amounting to 10 leagues, (p. 270)—so that without the safety-valves of volcanos the globe might possibly be shattered—would, probably, not be maintained now by many. It will be remembered that the researches of Mr. Hopkins indicated that precession would be greater than it is if the earth is not solid for at least 1,000 miles deep; though it may be partially fluid at less depths, provided the fluid spaces are insulated, or have only slight communication.

This volume is one of an extensive series called *Bibliothèque des Merveilles*, embracing a great variety of subjects.

Report of the Astronomer-Royal to the Board of Visitors of the Royal Observatory, Greenwich, read at the Annual Visitation of the Royal Observatory, 1877, June 2.

We give a few extracts from this report, which is always a very interesting document. The period comprised is between 1876, May 8th, and 1877, May 13th. Under the head of *Library* we read: "Our pursuits have ramified into more numerous branches than formerly (for instance, into telegraphy, photography, spectroscopy), and in each of them literature has become more extensive, and in consequence our purchases of books have become larger than heretofore." Concerning the astronomical instruments we learn that the Y of the W. pivot of the transit-circle had been cut, probably by a particle of sand [far more mischievous in the bearing of the instrument than in the observer's eye!] This was burnished down by Mr. Simms, and a re-determination of the form of the pivots shows that there is no sensible deviation from circularity in their form [a thing as important as it is difficult to secure]. The increasing stiffness of motion of the great equatorial has been remedied, and its motion is now very free. "A large direct-vision spectroscope has been quite recently made by M. Hilger, on a new plan, in which either great dispersion or great purity of spectrum is obtained by the use of 'half-prisms,' according as the incident-pencil falls first on the perpendicular or on the oblique face. In this spectroscope either one or two half-prisms can be used at pleasure, according to the dispersion required, and there is facility for increasing the train to three or four half-prisms, though the dispersion with two only is nearly double of that given by the large ten-prism spectroscope. The definition in this form of spectroscope appears to be very fine." The number of observations of all kinds made with the transit circle is 8763, this includes 3748 observations of the transit of separate limbs, and 3274 circle observations, each requiring a separate reading of the six microscope-micrometers. With the reflex-zenith-tube were made 33 pairs of observations, the instrument being reversed between the observations, and 10 single observations. With the altazimuth were taken 772 azimuths of the moon and stars, 399 zenith distances of the moon, and 524 azimuths and zenith distances of the collimating mark. With the transit-circle 85 places of the moon were observed, or 6·8 per lunation. With the altazimuth 172 places, or 13·8 per lunation. The great value of the latter instrument is thus apparent. Under the head of *Reduction of Astronomical Observations,* we find the colatitude for 1876 is 38° 31′ 21″·52 ; that for 1875 is 38° 31′ 21″·34.

The preparation of the Nine-year Catalogue is steadily advancing. It will contain more than 2,250 stars (a considerable number being circumpolar) and it is intended that it should be completed so as to appear with the volume of Greenwich observations for 1876. Under *Spectroscopic and Photographic Observations* is the following: "During the past year the sun's chromosphere has been examined with the spectroscope on 57 days, . . prominences were seen on 39 days, or about 75 per cent. of the whole number of days. . . At the end of May, 1876, spectroscopic determinations of the sun's rotation were made by observations of the relative displacement of the Fraunhofer lines at the east and west limbs respectively. The results are in close agreement with the value of the rotation found from observations of sun-spots. A similar determination has also been made in the case of Jupiter, with equally satisfactory results. . . Photographs of the sun have been taken on 140 days, and of these 239 have been preserved. The photographs show a complete absence of spots on 57 days, and on 29 of these there are neither spots nor faculæ."

Under the head of *Reduction of Magnetical and Meteorological Observations* we find that the principal results for magnetic elements in the year 1876, are the following: Approximate mean westerly declination, 19° 8′. Mean horizontal force, 3·898 in English units, 1·797 in metric units. The mean dip (to the nearest minute) shown by the 9-inch, 6-inch, and 3-inch needles, is 67° 41′.

"The mean temperature of the year 1876 was 50° 1′, being 0°·7 above the average of the preceding 35 years, the months of greatest deviation were

May, July and December, their temperatures being respectively 3°·5 below 3°·7 above, and 4°·0 above the average. The absolute temperature was seven times above 90°, the highest being 94° on July 17th, the lowest was 17°·4 on January 8th. The mean daily motion of the air was 291 miles, being 13 miles greater than the average. In February and March the movements were 54 miles and 113 miles above the average. The greatest day's motion was 869 miles on March 15th, the least was 49 miles on February 11th." . . "The computation of the photographic records of the barometer from 1854 to 1873 has so far advanced that we can assert positively that there is no trace of lunar tide in the atmosphere, but that there is a strongly marked semi-diurnal solar tide, accompanied with a smaller diurnal tide."

The Astronomer-Royal says: "The printing of the astronomical part of the volume for 1875 is complete, with the exception of the photographic and spectroscopic observations and the introduction. An appendix of astronomical character, I propose to attach, but at the end of the volume, the first part of Numerical Lunar Theory (already printed), including the general theory, extensive numerical developments of certain functions of the moon's co-ordinates, and the theory of symbolical variations." Part of the volume for 1876 is also in the press. "With the separate copies of the Astronomical Results it may perhaps be desirable to circulate copies of the Astronomical Introduction, a work which might almost be adopted as a practical text-book for observatories of the Greenwich class." We are glad to observe that it is proposed to print 350 copies of the nine-year star catalogue for separate distribution or sale, besides the 300 for the volume. "The general rule of distribution, which is now made on the responsibility of the Astronomer-Royal, is to present the entire volume to observatories, libraries, scientific academies, and other institutions of permanent character; the separate copies of Results to private observatories, or amateur astronomers." Observations of small planets are sent every quarter to the Paris Observatory, and other observations occasionally to the Royal Astronomical Society. . . Daily meteorological results are supplied to M. Le Verrier, to the War Department at Washington, to the Registrar-General, and to the Meteorological Office, and are made more quickly accessible to the public by newspapers, and by affiche on the Observatory walls."

Under the head of *Chronometers, Time Signals*, &c., the report says, "We have now under our care 191 chronometers. Of these 34 are box chronometers, the property of chronometer makers, and placed here for the annual competitive trial, the others . . are the property of the Government. . . No comparisons are made in artificially depressed temperature, but every chronometer is tried in a temperature raised nearly to 100° Fahrenheit." Every weekly rate is inspected by the Astronomer-Royal himself, who says, "The supplementary compensation, to which allusion was made in the last two reports, has now been repeatedly tried, and I think I may say that in the makers' hands it is successful. We do not doubt that chronometers are delivered to us by the makers in a better state of compensation than formerly. . . More than three-fourths of the chronometers now on trial are furnished with the new supplement. The accuracy of chronometers continues to improve, but very slowly, and not uniformly."

"The automatic drop of the Greenwich time-ball has failed only on seven days, when the ball was not raised on account of high wind, and on three days from accident. . . The Westminster clock, from some neglect of the agent of the chronometer-maker, has not fully maintained its character; it has sometimes been more than 3s. in error."

"Time-signals, originating at the Royal Observatory, and dispersed, without sensible loss of time, by the 'Chronofer' of the General Post Office, are distributed, I believe, with perfect accuracy through every part of the kingdom."

The *Personal Establishment* (the same as at the last report) consists of Mr. Christie, Chief and Confidential Assistant, and the Vicegerent, with fullest powers of the Astronomer-Royal in his absence. In the Astronomical Department, Mr. Dunkin superintends the Supernumerary Computers, and at present also the extensive calculations for the nine-year catalogue. "In recognition of Mr. Dunkin's high standing, and his close confinement to the work of computations, he is entirely relieved from active observations." Mr. Lynn, usually assisted by one supernumerary, is charged with the care of the Altazimuth, and the reduction of the observations made with it. Mr. Criswick has the management of chronometers and time-signals, the care of stationery and the quarterly accounts of the Observatory. He is aided by one supernumerary. Mr. Downing usually communicates with H. M. Stationery Office, and is charged with the care of Library and MSS. Mr. Thackeray's special duty is attention to the Chronograph and to Meridional Zenith Distances. He is assisted by one supernumerary. The ordinary astronomical observations are made, as far as possible, by the four gentlemen last mentioned. The supernumeraries are young men whose terms of employment are entirely at the discretion of the Astronomer-Royal. They are encouraged to learn the methods of observation, and are frequently able to relieve the regular assistants and to give efficient assistance to the Observatory. Mr. Ellis and Mr. Nash, as senior and junior assistants, superintend every part of the Magnetical and Meteorological Department, to which four young men are at present attached. Mr. Maunder continues to control the Spectroscopic and Photographic Department, one supernumerary is under him. The nature of his employment causes that he becomes in fact the superintendent of the South-east Equatorial. Belonging to the Observatory are also a gate-porter, a watchman, and a labourer. Carpenters and other workmen are almost always employed. Work of every kind—observational, scientific, practical—is every day recorded in sheets or books, which are submitted periodically to the inspection of the Astronomer-Royal.

Under the head of *Extraneous Work:*—As regards the operations for the transit of Venus, the computing staff under Captain Tupman has by degrees been reduced to two junior computers within the observatory, and one or two computers external to the observatory, who are employed on large groups of systematic calculations, . . and the Astronomer-Royal thinks that he will soon be able to offer the first determination of correction to parallax from eye-observations of the transit. "The method of determining the geographical longitude of the principal station in each group by vertical transits of the moon, has been found very successful at Honolulu and Rodriguez. For stations in high south latitude, horizontal transits are preferable. . . The corrections to the moon's tabular places have been determined with much care from meridional observations at the principal European observatories." The differences of longitude or the relations of clock-times, within the groups of stations are ascertained. These calculations must be followed by the preparation of the factors of errors of various elements. Little progress is made in these, the work will not be heavy. No further advance is made in the photographic reductions. The work is large, but it is simple and will not be oppressive."

"As regards the Numerical Lunar Theory—In the algebraical theory an alteration has been made by the substitution of radial forces for the equation of Vis Viva. Nearly all the numbers had been computed, and the additional numerical operation was small. The numerical calculations of the factors of symbolical variations are advancing, and the computations of the perturbing side of the equations, with due attention to the terms requiring extension of decimals, are in progress. The numerical errors to which I alluded in the last report are corrected, and I do not think that any systematic error now remains. With the view of preserving, against the ordinary chances of destruction or abandonment, a work which is already one of considerable magnitude, I have prepared and have printed as Appendix to the Greenwich Observations (with additional copies as for a separate work) the ordinary equations of lunar disturbance, the novel theory of symbolical variations and the numerical developments of the quantities on the first side of the equations. The last of these will ultimately require some additions for the terms whose magnitude is increased (in algebraical development). The work is perhaps somewhat larger than I anticipated, and the regularity of its progress has been disturbed by very frequent interruptions of my own attention, occasioned chiefly by annoying occupations on the transit of Venus. I trust that it will in future go on in a more orderly and more rapid way."

We have transcribed these particulars of this great work on account of its importance, and because they will be acceptable to such as interest themselves more or less in the more recondite departments of astronomy. It might be thought that a work of such magnitude and complexity would more than sufficiently occupy the time of one who had little else to attend to, yet we see that the Astronomer-Royal has taken it upon himself in addition to his manifold regular engagements, and for some time past, with the extra serious demands made by the calculations, &c., about the transit of Venus. We offer our warm congratulations to Sir George Airy that he has got on so far with the Numerical Lunar Theory, and trust that life and health may enable him to complete it. We are very glad to find that it is to appear also in a separate form.

We now come to the concluding *General Remarks*. "The subject which, I think, must first present itself to the mind of any one who has traced the history of the Observatory, is the increase in the number and the fulness of our occupations. Of those, one in particular (altazimuth observations of the moon) has originated with myself, others from the suggestions of the Board of Visitors, or from the obvious demands of the scientific world. This increase is felt even in our buildings and grounds; every corner of every room is or will shortly be occupied, and the form of the ground almost forbids extension. The printing of the steps of the reductions of observations (which originated with myself more than 40 years ago) naturally increased the labour within the Observatory, as well as the expenses without it. This printing, however, must never be abandoned. But there is another part of which the policy still appears to me somewhat doubtful, namely, the printing in extenso of every figure of original observations, it being remarked that the originals or extracts are always open to astronomers. I brought the question of suppressing these before the Board of Visitors many years ago, but the opinions of astronomers (I cite in particular the honoured name of M. Biot, and that of Mr. Johnson) were so strongly adverse to it that I laid aside all further thoughts of it, and I do not even now profess to entertain a decided opinion. The three points, however, to which I have alluded (the extent of scientific occupations, the enlargement of buildings, and the amount of printing) must before long engage the attention of the Visitors.

"Royal Observatory, Greenwich: G. B. AIRY."
"1877, May 14."

Our readers will be glad, we think, to have these latest details of this noble establishment, of which severe conscientiousness, unwearied and unsparing painstaking, directed by consummate skilfulness, and an admirable organisation, have long been characteristics, and of which indeed they are stereotyped features. In conformity with ancient traditions its main strength, it is seen, continues to be thrown upon meridional work. But heavy as this is, we perceive how great and apparently likely to increase are the range and variety of other-regular occupations, to say nothing of extraneous work, which of late must have been peculiarly onerous. We are sure we express the sincere hope of all who cultivate astronomy, and many others besides, that the want of more money, more men, and, if possible, more space, may not ever circumscribe the operations of the Royal Observatory, when any one or all of these shall be considered by its illustrious chief to be essential to its continued efficiency. In his 2nd Isthmian, Pindar says that the Muse was become mercenary, her songs were sold, and they had "silver in their faces," that is (apparently) they were cold and forced, rather than effusions of genuine warmth and inspiration. Terpsichore may sometimes have been venal, but we greatly doubt that Urania was ever liable to this charge. Her votaries (though sometimes but poorly rewarded in a pecuniary sense) work for the love of her, and uncomplainingly sacrifice much of their ease and comfort, often, too, of their substance. We cannot perceive any of that silvery hue of which the great lyrist spoke in the splendid volumes which annually issue from the Royal Observatory. Taken in conjunction with the reports, the labour of their production is seen to be a labour of love, and those whose pains contribute to them we think must be credited with something more than diligence in a mechanical routine of duties. A higher motive than temporal gain (were it even large enough to be attractive) must keep unimpaired the freshness of consecration to science from year to year of so many daily and nightly hours spent in observing and calculation. The Annual Visitation of the Observatory is an excellent feature; besides other advantages, it has once more put us in possession of a report which will not only raise (if possible) the already high estimate of the place in those best capable of appreciating it, but deepen the thankfulness of the humblest star-gazer that we have such an institution, presided over by such a head, and served by such a staff of able and zealous astronomers.

Report of the Savilian Professor of Astronomy to the Board of Visitors of the University Observatory, Oxford, June 19th, 1877.

The visitors of the University Observatory are the Vice-Chancellor, the Proctors, the Astronomer-Royal, the Director of the Cambridge University Observatory, the Radcliffe Observer, John A. Dale, M.A., Balliol College, Bartholomew Price, M.A., Fellow of Pembroke College, William Esson, M.A., Fellow of Merton College, and Warren De La Rue, M.A., New College.

The observatory has been in active operation not quite two years. The report embraces the period between June, 1876, and June, 1877. Owing to derangement of the sun spectroscope, observations of the chromosphere have been interrupted. 652 photographs of the moon have been taken with the De La Rue Reflector, to most of which micrometric measurement will be applied when the costly micrometer constructing by Mr. Simms—the gift of Dr. De La Rue to the observatory—is ready. The Savilian Professor hopes then to enter upon the difficult and interesting research on the physical libration of the moon. 259 complete measures of 117 double stars have been taken with the great equatorial. The elliptical orbits of several of these have been approximately determined by the graphical process of the late Sir John Herschel; but Professor Pritchard has devised a subsidiary method, by which a direct mathematical computation is superadded to the former. In this way elements have been obtained of the orbits of 70, Ophiuchi; ξ Ursæ Majoris, and ω Leonis, which appear to represent the observations more accurately than any orbits heretofore determined. "This process is necessarily laborious, but in the course of time I hope to apply it to the cases of all other binary stars, which at present hold out any reasonable hope of success. The orbit of μ^2 Boötes is now in hand. Three new comets have been discovered during this year, viz., those of Borelli, Winnecke, and Swift. The first was observed here seventeen times, the second forty-eight times, and the third twenty-three times. From these instrumental observations the elements of the orbits and ephemerides of the three comets have been computed and published with, I believe, at least as much promptitude and accuracy as will be found in the work of any other existing observatory. Investigations of this description I regard as an especial element in the astronomical programme of our new institution. When it is remembered that each complete ephemeris requires the accurate use of about forty-five thousand figures, the labour bestowed on the work will become apparent: I say nothing of its intricacy."

A second set of observations of six of the satellites of Saturn has been completed this year. They comprise measures of the co-ordinates of Titan, Rhea, Iapetus, Dione, Tethys, and Enceladus. A third set, it is hoped, will be obtained in the autumn. Their value, it is believed, is considerable. The sun's chromosphere has been observed and delineated on twenty-two days, when the spectroscope became deranged . . . . The sun's photosphere was diligently searched for *Vulcan* on March 21, 22, and 23, but no intra-mercurial planet was seen.

Referring to lectures given during the academical year to the classes of University students, Professor Pritchard mentions one subject as the lunar theory; and we are glad to learn that he has printed the notes of these lectures, which, we think, would be acceptable also to persons out of the university. In regard to the numbers of the classes (21, 8, and 12, respectively), the Savilian Professor remarks:—"I fear that the very numerous material advantages offered to the students in so many other branches of academical study, overweighted and prudentially deterred them from bestowing time and thought on this sort of not immediately fructiferous knowledge. The time, I think, is fast approaching when some small modicum of material encouragement may be extended also to a science which from remote ages has been a pioneer of civilization, is inferior to no other science in practical utility, and is, probably, superior to all in its educational value to the higher class of mathematical students." . . . . "By the recent grant of £2,400 for the erection of a lecture theatre and students' library, the University of Oxford has taken the lead in England, and I think I may say in Europe, in the realization of what should be the ideal of an astronomical observatory in a national university, viz., an institution provided with suitable means for original research, combined with the appliances required for the education of its students." The main deficiencies in the University Observatory, we are told, consist in the want of a good meridian instrument, and a library of reference: and we rejoice to find that the Board of Visitors are taking into consideration the various suggestions made in this report.

A MS. note by Professor Pritchard, appended to the report, explains why nothing was said about preparations for observations of Mars at his opposition in September, for the determination of solar parallax. It appears that no instrument, excepting the heliometer, seems to exist adequate to the exact measurement of angles such as those of 40 minutes of arc; and even that instrument is replete with imperfections and difficulties. Since the printing of this report a micrometer, devised by the Professor, is being constructed, which is likely to measure distances to one-tenth of a second. If this little instrument, to be called a duplex heliometer eye-piece, succeeds, it promises, he thinks, to be a boon to astronomers. "We hope," he adds, "to use it on Mars, on the satellites of Saturn, and on the moon for the verification of the Lunar photographs as measured by Dr. De La Rue's beautiful micrometer." The Professor closes his report with the expression of his satisfaction in regard to the intelligence and success with which his two assistants, Mr. Plummer and Mr. Jenkins, have discharged their, not unfrequently, laborious duties; and the skilled mechanic, Mr. John Mullis, who assists as observatory attendant, receives the acknowledgment of his steady and efficient services.

A short account of the origin of this observatory was given by Professor Pritchard at the meeting of the R. A. S. December, 1873 (see *Astronomical Register*, January, 1874), and now, through the unsparing liberality of the University, the zeal of its scientific professors, and the munificence of Dr. De La Rue, we have in this report such evidence of its vigorous infancy, that we may confidently expect that its object as a first-class observatory for astronomical physics will be fully realised; and that as an institution for the education of astronomical students, it will be second to none other. We look forward with interest to the appearance before long of the first volume of its annals (alluded to as a possibility in the report); and we share—doubtless, with all astronomers—the satisfaction with which the Savilian Professor, and all who have assisted him, must regard the rapid accomplishment of such great success.

Report of the Radcliffe Observer to the Board of Trustees, read at their meeting, held on July 7th, 1877. [Printed by order of the Trustees, for Private Circulation.]

The Report embraces the year ending June 30th, 1877. Of transits, 2,273 have been taken, and 3,376 zenith distances. The number of stars observed is 1,099. The surface of the sun has been examined and delineated on 144 days. All the astronomical reductions are made for 1875, and the printing of this portion of the volume is nearly completed. Considerable progress has been made in the compilation of the *Third Radcliffe Catalogue*, which will contain 4,389 stars, and will be, it is hoped, a valuable contribution to sidereal astronomy. Mr. Main speaks of the "unflagging industry and zeal of every member of the establishment." The chief instruments are a heliometer with 7½-inch object-glass, and a transit circle with telescope of 5-inch object-glass, which have been employed for many years in a way, it is hoped, which will not be without its effect on the astronomy of the age. "The physics of astronomy," remarks the Radcliffe observer, "may be safely left to other observatories, equipped and designed for that purpose, and especially to that recently attached to the University of Oxford, and directed with such good effect by the Savilian Professor." The personal establishment remains the same; Mr. John Lucas, first assistant, Messrs. Fred. and Henry Bellamy, second and third assistants, and Mr. Luff, who takes a very important part in the computations of the observations. Such are a few excerpta from this report; the perusal of which cannot fail to give pleasure and satisfaction to all interested in the science, and to confirm their high appreciation of the establishment, its eminent head, and his meritorious and hard-working assistants.

EXPLANATION OF NAMES ON MAP OF THE MOON.

(Concluded from page 13.)

Rabbi Levi. R. Levi Ben Gershon. (Ralbag.). Spanish Jew. Commentr., math., astron., &c., d. 1342 (cir.)
Regiomontanus. John Müller. Archb. of Ratisbon. Astron. and mechan. d. 1476.
Reichenbach (M.) Optician, d. 1826.
Reiner. Genoese monk; pupil and friend of Galileo.
Reinhold. Germ. astron.; friend of Copernicus, d. 1553.
Repsold (M.) Germ. optician, 19th century.
Rheticus (M.) Germ. astron., d. 1576.
Rheita. Astron., d. 1645.
Riccioli. Ital. philos. and astron., d. 1671.
Riccius. Astron., d. 1521.
Riphæan Mts. (M.) According to the Greeks, in northern regions of the earth; sometimes called Hyperborean.
Ritter (M.) Germ. geographer, d. 1859.
Römer. Danish astron., d. 1702.
Roris Sinus. Bay of Dew.
Rosenberger (M.) Germ. astron., 19th century.
Explanation wanted: Rocca (R.), Rook Mts. (S.), Röst (S.).

Sacrobosco (Holywood.) Math., d. 1256.
Saussure (M.) Genevese naturalist, geologist, &c., d. 1799.
Scheiner. Germ. math. and astron., d. 1650.
Schickard. Astron. and oriental., d. 1635.
Schiller. (Cœlum Stellatum Christianum) 1627.
Schubert (M.) Astron., 19th century.
Schumacher (M.) Germ. astron., d. 1850.
Seleucus. Q. Nicator, first king of Syria, d. B.C. 280 (?).
Serenitatis Mare. Sea of fair (or clear) weather.
Sharp (H.) Math., astron. and optician, d. 1742.
Short (S). Optician, d. 1768.
Shuckburgh (L.) Math. and astron., d. 1804.
Silberschlag (M.) Germ. math. and mechanist, d. 1791.
Snellius. Dutch math. and philos., d. 1626.
Somnii Palus. Marsh of Sleep.
Somniorum Lacus. Lake of Dreams.
Sosigenes, of Alexandria, B.C. 50.
Stadius. (Stadt.) Dutch histor. and astrologer, d. 1579.
Stevinus. Dutch math. and mechanician, d. 1633?
Stöffler. Germ. math. and astron., d. 1531.
Strabo (M.) Gr. geogr., d. after A.D. 21.
Street (S.) Astron., d. 1661.
Sulpicius Gallus. Roman Tribune and Astron.; predicted eclipse of the Moon, B.C. 168.
Explanation wanted: Santbech (R.), Schomberger (R.), Segner (S.), Simpelius (R.), Sirsalis (R.), Sömmering (M.), Stiborius (R).

Tacitus. Rom. Histn., after A.D. 117.
Taquet (S.) (Tacquet). Astron., d. 1660.
Taurus Mts. In Asia Minor.
Taylor (M.) Q. Brook Taylor, math. d. 1731 (?)
Thales, of Miletus, philos., d. B.C. 546.
Theætetus. Disciple of Socrates. Geometrician.

Thabet. Physn., geom. and philos. Bagdad, d. 901, his grandson also Physn. and Philos., d. 973-74.
Theon, Sen., of Smyrna. Contemp. of Ptolemy.
Theon, Jun., of Alexandria (father of Hypatia).
Theophilus. Alexandrian School.
Timœus, of Locri. Pythag. Philos. Friend of Plato.
Timocharis. Alexandrian School. B.C. 300?
Tobias Mayer (S.) Germ. astron., d. 1762.
Torricelli (M.) Ital. Philos. and math., d. 1647.
Tralles (M.) Q. Tralles of Berlin, 19th century (?).
Tranquillitatis (Mare). Sea of Tranquillity.
Triesnecker (L.) Astron., d. 1792.
Explanations wanted: Tannerus (R.), Taruntius (R.).

Ulugh Beigh (M.) Tartar prince, astron., Samarkand, d. 1449.
Explanation wanted: Ukert (M.).

Vaporum Mare. Sea of Vapours.
Vega (M.) Germ. math., d. 1802.
Vendelinus. Astron., d. 1626.
Vieta. Fr. math., d. 1603.
Vitello. Polish math., d. 1254.
Vitruvius. Rom. architect, time of Augustus Cæsar.
Vlacq (M.) Dutch math. and printer, 1630.

Walther. Of Nuremberg (pupil of Regiomontanus), d. 1504.
Wargentin (S.) Swedish math. and astron. d. 1783.
Weigel (S.) Germ. math. and astron., d. 1699.
Werner. Germ. math., d. 1528.
Wichmann (N.) Astron., d. 1859.
Wilson (S.) Q. Of Glasgow, 18th century. (?)
Wolff Mt. (S.) Germ. math. and philos. d. 1754.
Wurzelbauer (S.) Germ. astron. d. 1725.
Explanation wanted: Wilhelm I. (R.)

Xenophanes. Gr. philos. and poet, B.C. 540—500.
Zach (M.) Hungarian astron. d., 1832.
Zucchius. Zucchi, Ital. astron., 17th century.
Zupus. Zuppi, 17th century. (?)
Explanation wanted: Zagut (R.)

CORRESPONDENCE.

N.B.—We do not hold ourselves answerable for any opinions expressed by our correspondents.

To all communications must be annexed the name and address of the sender, as a guarantee of good faith.

TO THE EDITOR OF THE ASTRONOMICAL REGISTER.

"MIRACULOUS VISION."

"*Τὸ γὰρ ψευδὲς ὄνειδος οὐ περαιτέρω τῆς ἀκοῆς ἀφικνεῖται.*"—*Æschines.*

Sir,—Translating Mr. Sadler's euphemistic utterances in the November number of the *Astronomical Register* into plain English, they really amount to nothing more nor less than a charge of *lying* against either the Baron

von Ertbon or his nephew. As I happen to know that the Baron himself is a constant reader of the *Register*, I think it possible (although not probable) that he may notice the impeachment of his, or his nephew's, veracity. Whether he does so or not, however, perhaps I, who am responsible for the publication in England of some of his experiments, may venture to suggest that their astonishing results may conceivably admit of some other interpretation than that of actual deliberately uttered falsehood or conscious deception.

A word, however, in the first place, as to the comites of Polaris. Writing *currente calamo*, I was under the impression that observations of these minute bodies had been published in some of our scientific periodicals. If though this be not so, then I must say that Mr. H. J. Slack, F.G.S., &c, of Ashdown Cottage, Forest Row, and Mr. C. L. Prince, F.R.A.S., of the Crowborough Observatory, both in my own county, are the only two persons I know who have seen them. I have failed to do so myself with the object-glass of which Mr. Sadler speaks, whose aperture, by the way, for his own satisfaction I may inform him is precisely 4·2 inches. Perhaps even he though will admit that it was a most wonderful coincidence which enabled a lad of 13 to imagine (or invent) two comites in the position in which they were detected by their original discoverers.

The weather here has been so frightful since I read Mr. Sadler's letter that I have had no chance of personally examining any of the objects of which I gave a list from the Baron von Ertbon's letters. It strikes me, however, in the outset, that "the companion to μ^2 Herculis," can scarcely mean the excessively close component of μ^1. I need scarcely remind Mr. Sadler that they are two totally different stars, of which the latter is a binary system, the first only optically double. Dividing power is strictly a function of aperture, and has very little indeed to do with keenness of vision. As a matter both of theory and fact no 4·2-inch object-glass could separate any pair of stars closer than 1″·05. It may be, however, that M. von Ertbon's nephew would describe as two stars what he could perceive not to be a single one, and that this may furnish an explanation of the case of Struve 2690. It must be borne in mind that the boy is practically ignorant of astronomy, and is merely taken to the telescope and told to describe what he sees, its name in every case being purposely concealed from him. As his uncle writes to me, "Ces faits je les considère comme positif, car je me suis entouré de toutes les précautions possibles."

As Mr. Sadler says nothing about Mr. Ward, I presume he is not disposed to throw any doubt upon that gentleman's most astonishing visual powers. It may then interest him to know that I presented some observations of Mr. W.'s on the satellite of Neptune, made with a 4·3 inch Wray object-glass! to the Royal Astronomical Society on Friday last. As the evening was, however, mainly taken up by analysis and papers on Mars, it (in common with a large number of other papers) was never read at all.

Forest Lodge, Maresfield, I am, sir,
Uckfield: Nov. 12, 1877. Your most obedient servant,
WILLIAM NOBLE.

[We have a similar letter from Baron von Ertbon, Conseiller Provincial, Anvers, complaining of the expressions of our correspondent. The Baron says that "his friends, his family, and self have often confirmed the fact that his nephew had counted the larks before other people had seen the flock."—EDITOR.]

THE SPECTRUM OF RAIN.

I have read Professor Piazzi Smyth's papers on Optical Spectroscopy as applied to meteorological purposes with much interest. If approaching changes in the weather can be foretold by less skilled observers with a waistcoat-pocket spectroscope, such instruments, costing as they do little more than a guinea, will come into use generally as home and travelling companions. I would, however, suggest that it would simplify the matter if the Professor would supplement his diagram with a representation of the spectra of dry and rainy weather as they really appear in the instrument. Figures in which height does not mean height but depth of tint, and a sloping top indicates shading off, though a very fit and comprehensible method of explaining the changes to persons who are familiar with such modes of treatment, are puzzling to general readers, who require to be shown exactly what they may expect to see. Also the low spectra are divided into "wet and warm," or "dry and cold;" but how is it when the weather is wet and cold or dry and warm? Such conditions must obtain sometimes, and I fancy would be intermediate in character, the former divisions giving the extremes. In my own observations with a waistcoat-pocket instrument, that is to say, the prism and slit portion of Browning's "amateur star spectroscope," I never fail to see a "rain-band," there always being a marked shading on the less refrangible side of D; the band is not always equally conspicuous, but it is invariably there. Perhaps my *low* view being over the channel, aqueous vapour is never wanting. It is unfortunate that that portion of the spectrum is so difficult to photograph, or useful records might be made of various states of weather for comparison with observations.

GEORGE GUYON

CONJUNCTION OF MARS AND SATURN.

When viewing Mars and Saturn with a low power near the time of the recent conjunction, I was struck with their resemblance to a pair of coloured double stars, say β Cygni, the brilliant Mars looking like an orange-coloured primary, and giving by contrast a blue tint to less conspicuous Saturn. The two planets were well within the field with a power of 220. G. GUYON.

FRACASTORIUS.

Much has been said in these pages from time to time concerning this interesting object. I am not going to attempt to describe the whole of it now, but shall only say a word or two about an object just off the north-east extremity of its horseshoe-shaped wall, designated IV. B ρ 6, No trace of it is shown in B. and M.'s *Mappa Selenographica*, but it appears distinctly marked in Neison's map, and is not at all a difficult object with a 2·25-inch achromatic, when seen under suitable illumination. It is a curved ridge, apparently the boundary of about one half (the eastern) of an oval—or, more properly speaking, egg-shaped—plain, having its more rounded extremity northwards. Its form alone suggests that it is part of the wall of a ringed plain of other days; and on three or four occasions, when seen under suitable illumination (notably on 1876, Sept. 6th, 11h., when the terminator passed between Messier and its companion A), and using the little 2¼-inch Browning achromatic, appearances have presented themselves that led me to believe that there *was* a ring. In a letter to the *English Mechanic*, vol. XXIV., p. 480, I asked if some observers using tolerably large apertures would kindly examine this object. Whether this has been done or not I do not know, no tidings of any such examination have ever reached me. On 1877, September 26th, 11h., I made a careful survey of Fracastorius with a 9¼-inch "With" mirror, of which a gentleman kindly granted me the use. The terminator was then passing along the west walls of Chacornac and Fracastorius, and through the bright little crater Rosse. Under that illumination an immense depression was revealed, of which the ridge IV. B ρ 6 was the eastern wall, but without any raised wall on the west. A little south of the centre of this depression a small craterlet was situated, at the southern end of a very slightly curved and likewise short ridge. If some of the readers of the *Astronomical Register* who are armed with large instruments would direct them to this object during the next month or two, they would be doing useful work.

Southampton, FRANK C. DENNETT.
Nov. 9, 1877.

THE NOVEMBER MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Sir,—I am a mere amateur, a plain man, earnestly and sincerely devoted to the study of astronomy, and attend the meetings of the Royal Astronomical Society (of which I have had the honour to be elected a Fellow) with tolerable frequency. With other members I went down last night, the opening one of the session, to hear what had been done in the recess, and was pleased to listen to the reading by Mr. Glaisher of a long list of papers whose titles gave earnest of their great interest. I settled myself in my seat anticipating an evening's treat—and this is what happened: I do not wish to speak disrespectfully of our Council, but they seemed determined that no one else should get a word in edgeways. For one hour and twenty-five minutes by the clock in our meeting-room, were we regaled with analytical formulæ, &c., &c., which, however interesting and intelligible when studied in print, merely involve a waste of precious time when read out. I saw with my own eyes one of our greatest mathematicians sleeping the sleep of the righteous during this interval. The interest of the meeting generally began to revive when Lord Lindsay gave some news from Ascension relative to Mr. Gill's expedition, and decidedly increased when Mr. Green was called upon by the President to describe a really magnificent series of drawings of Mars which he had made; a call, however, coupled with a request to be as brief as possible! Mr. Green, whom we wanted to listen to, had to scuffle through his paper, Captain Noble being asked to read some notes of his on the planet, had nothing to say (and said it); and then we were treated to an exhibition which struck many of us as being most singularly *malapropos* in a great scientific society. It took the form of a paper on Mars, which was nothing more nor less than a succession of gross, incoherent nonsense; and this by a gentleman who is supposed (by the outside world) to represent astronomy! Well might the President forbid discussion upon such a paper (ostensibly for want of time) and order us at once to proceed to the ballot.

But is this all quite fair to the mass of us, the Fellows of the Society at large? Here are the most long-winded speeches made, unchecked by the President, until more than three-parts of the evening are gone, and then the little slice of meat in the sandwich, between pure mathematics and mountebankery, is shaved to an infinitesimal thinness, and "no discussion can be allowed." Perhaps the dignity of the Society *may* be maintained by what happened at the beginning of its proceedings—it can hardly fail to suffer, however, most seriously, by what was permitted to happen towards the end of it. I am, yours truly,

November 11, 1877. X.

MERCURY AND ITS TRANSITS.

The clear horizon several evenings at the end of April gave a ready opportunity of perceiving this fickle little planet with unaided vision. He appeared well-deserving the old epithet στιλβων : more conspicuous than Aldebaran in the evening twilight, inferior to α Orionis, but these stars were not in the glow as the planet was. Next spring Mercury will also be very favourably situated in this respect previously to being seen on the sun's disc. The transit of May 6 is of longer duration than any that has taken place since 1753. The transit of 1677, which is noted as having set Halley thinking about getting the solar parallax by those of Venus, was also observed in this country. Flamsteed's friend, R. Townley, in Lancashire, observed egress at 2h. 54m. 0s. Halley observed this transit through at S. Helena. Flamsteed in his *Historia Cœlestis* remarks of it—"In insula Helenæ ubi tunc fixarum australium observandi causâ commorabatur D. Halleius observavit quod 9h. 26m. 17s. mane, limbus solis a Mercurio temeratus. 2h. 41m. 54s. solis limbus factus integer."

On making some approximate calculations lately it appeared that Mercury will be seen four times from ingress to egress on the sun's disc, at Greenwich, in the next century, viz., in 1907, 1914, 1937, 1973, whereas in the present century only one transit, that of 1832, was visible there throughout. For the next half-century they happen as follows :

1881, Nov. 7.	Below horizon.	
1891, May 10.		Egress about 4¾h. morn.
1894, Nov. 10.	Ingress about 4h. aft.	
1907, Nov. 12.	„ „ 10¼h. morn.	„ „ 1¾h. aft.
1914, Nov. 6.	„ „ 9¾h. morn.	„ „ 2h. aft.
1924, May 7.		„ „ 5½h. morn.
1927, Nov. 8.		„ „ 8¼h. morn.

Upton Helions Rectory, Crediton : S. J. JOHNSON, F.R.A.S.
July 12.

BRILLIANT METEOR.

About 8·25 p.m., Nov. 23rd, a most brilliant meteor was seen here. Being inside the equatorial room observing the shadow on Saturn, the meteor was not at all well seen. The sky was lit up very suddenly, and on turning to the west window the fragments of the explosion were seen falling in the sky, the colours being red and green. The west dome prevented my seeing the paths well, or indeed at all. The light which illuminated the observatory was green. No sound was heard.

Nov. 22, 4·10 p.m. A very vivid flash of lightning; an open book looked quite green.

Mr. Edward Crossley's Observatory, JOSEPH GLEDHILL.
Bermerside, Halifax.

SHADOW OF TITAN.

Dear Sir,—Yesterday evening, thanks to the untiring zeal of Mr. Marth, I succeeded in observing the transit of the shadow of the Titanian Satellite across the disc of Saturn.

On 23rd Nov., 23h. 10m., local sidereal time, the shadow had not reached the central meridian ; at 23h. 20m. I judged it to be on the meridian ; and at 23h. 30m. it was decidedly past it.

Therefore I estimate the mid-transit to have taken place on 23rd Nov. 23h. 20m. local sid. time, equivalent to 23rd Nov. 7h. 34m. G.M.T.

As to the distance of the shadow at mid-transit from the centre of the disc—I estimated the shadow to be somewhat, but very little, nearer to the projection of the ring on the disc, than to the (apparent) upper edge of the disc.

Now the radius of the disc was 8″.0, and the projection of the ring was 1′.3 apparent north of the centre; so the space to be divided by estimation was 6′.7, and the distance of the shadow from the projection was less than half this; let us say 3′.3, and therefore the distance of the shadow from the centre of the disc was 4′.6.

At moments the shadow was seen as a hard sharp well-defined black spot, but such views were only instantaneous and rare; at other times, at intervals of only a few minutes, it was all but invisible.

I also witnessed, in an inverting telescope, two superior conjunctions of Titan with the preceding ansa.

By "superior conjunction," I mean that Titan appeared to be then in a perpendicular to the line of ansæ, erected at the preceding ansa, and that he was apparent north of the said line.

The first conjunction occurred Nov. 7th. 9h. 51m. G.M.T.
The second Nov. 23rd 8h. 44m. G.M.T,

At the first conjunction, I unfortunately, through ignorance, did not look out for the shadow, and therefore overlooked it, as so many things have, from the same cause, been overlooked.

I thought I could see the disc of Titan about 0″.5 diameter.

I am, faithfully yours,
Sherrington Bray, Nov. 24th, 1877. WENTWORTH ERCK.

TRANSIT OF SHADOW OF TITAN

Across the disc of Saturn, 1877, November 23rd.

The transit of the shadow was well seen here, the time of the middle of its passage being as follows:

Greenwich M. T.	Telescope.	Observer.
h. m.		
7 35	12¼ Browning Reflector	N. J. Carpenter.
7 32	6-in. Simms refractor	J. G. Lohse.
7 27	15-in. reduced 10-in. Grubb	Ralph Copeland.

The shadow was 4″·78 south of the major axis of the ring, according to five measures with the Grubb refractor. The images with this telescope being steadier and more distinct when the aperture was reduced to 10-in. than when the whole object-glass was used, the focus remaining unchanged.

Lord Lindsay's Observatory, Dunecht,
Aberdeen: Nov. 26, 1877.

DISCOVERY OF A PLANET.

Despatch from Washington to the Observatory, Berlin.

Prof. Watson, of Ann-Arbor announces the discovery on the 12th November, 1877, of a bright planet of the 11th magnitude, in 4h. 20m. right ascension, 23° 55′ north declination, with a daily motion of five minutes south.—JOSEPH HENRY, Sec. Smithsonian Institute.

Berlin: Nov. 14, 1877. V. KNORRE.

(From *Astronomische Nachrichten*, No. 2167)

DISCOVERY OF MINOR PLANETS.

(175) Idunna, by C. H. F. Peters, Clinton, New York.

1877	M.T.	α (175)	δ (175)	(log. p.″ Δ)	
	h. m. s.	h. m. s.	° ′ ″		
Oct. 14	12 39 51	1 6 4·73	+8 6 37·4	0·294	0·710
16	13 16 20	1 4 48·89	+7 39 10·6	0·502	0·720

Clinton, New York: Oct. 17, 1877.

Planet (176), by Monsieur Henry, at Paris.

1877		α (176)	δ (176)	Mag.
		h. m.	° ′	
Nov. 6		2 32	+17 15	10
7	M.T. Berlin 13h. 40m.	2 30·37	+17 7·5	10·4

Planet (177) by M. Palisa, Director of the Pola Observatory.

1877			(α 177)	(δ 177)	Mag.
			h. m. s.	° ′	
Nov. 6	12h.	M.T. Pola	2 42 20	+15 20	11·5
7	14h. 45m.	M.T. Berlin	2 41 9	+15 15	11·5

The most recent position of (175) is as follows:

(175)

Oct. 27 M.T. Pola	α (175)	δ (175)	Mag.
9h. 24m. 50s.	0h. 58m. 47·43s.	+5° 20′ 23″·6	11·3

(From *Astronomische Nachrichten*, Nos. 2164 and 2166).

THE LATELY DISCOVERED ARABIC GLOBE.

Cavalier Meucci writes in *La Nazione*, 31st March, some further particulars of this interesting relic. It was so oxidised that it could scarcely be recognised as celestial. Cavalier Meucci sought out Regulus, which, being on the ecliptic, serves well to ascertain the age of such instruments. He found by this that the globe was constructed between the 10th and 11th centuries. He then resolved carefully to clean it, in order to verify the number and arrangement of all the stars and constellations, and to find its precise astronomical epoch. It appeared clearly that it had been calculated for 1075; 14° 10′, according to Albategnius, having been added to Ptolemy's places of the stars, at the rate of 1° for 66 years. The Cavaliere says—"It is wonderful with what precision the figures of the constellations are engraved; all the stars of the 6 magnitude being distinctly laid down, all of which, formed and unformed, I have examined one by one; and it is of great importance that this truly represents the order and arrangement given by Ptolemy. I was able, also, in spite of the difficulty of the Cufic letters, to find the signification of the names of the constellations; but there remained to translate an inscription round the Arctic circle, which I rightly imagined mentioned the author and the exact date of the making and finishing of the globe. The able Orientalist, Professor Lasinio, of our Higher Institute, kindly undertook the translation. It mentions that the globe was made in Valenza, in 1081, by Ibrahim Ibn Said Assahli, together with Mohammed, his son. . . . I think I may say that it is the most ancient of the four or five whose existence is known to me—*i.e.*, at Velletri, Dresden, London, and Paris."

ASTRONOMICAL OCCURRENCES FOR DECEMBER, 1877.

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Mars.
Sat	1		Sidereal Time at Mean Noon, 16h. 41m. 47·41s.	2nd Tr. I.	5 15	7 1·8
Sun	2		Sun's Meridian Passage 10m. 18·05s. before Mean Noon			6 59·6
Mon	3			2nd Ec. R.	4 8 58	6 57·5
Tues	4	10 3	● New Moon			6 55·4
Wed	5	10 10	Saturn at quadrature with the Sun Conjunction of Moon and Mercury 2° 12′ N.	3rd Ec. R.	4 48 23	6 53·2
Thur	6	9	Conjunction of Moon and Jupiter 3° 37′ N.			6 51·1
Fri	7					6 49·1
Sat	8	11	Conjunction of Moon and Venus 0° 42′ S.			6 47·0
Sun	9			1st Tr. E.	5 14	6 44·9
Mon	10					6 42·9
Tues	11	23	Conjunction of Moon and Saturn, 4° 16′ S.			6 40·8
Wed	12	9 34	☽ Moon's First Quarter			6 38·8
Thur	13	10	Conjunction of Moon and Mars 3° 46′ S.			6 36·8
Fri	14	16	Conjunction of Jupiter and Mercury 2° 11′ S.	Jupiter too near the Sun.		6 34·8
Sat	15	8 18	Near approach of 104 Piscium (6½) Illuminated portion of disc of Venus=0·512 Illuminated portion of disc of Mars=0·874			6 32·8
Sun	16	9 49 10 18	Occultation of 26 Arietis (6) Reappearance of ditto Saturn's Ring: Major axis=38″·48 Minor axis=2″·34			6 30·8
Mon	17		Sidereal Time at Mean Noon 17h. 44m. 52·35s.			6 28·9

DATE.		Principal Occurrences.		Jupiter's Satellites.		Meridian Passage.
		h. m.			h. m. s.	h. m. Mars. —
Tues	18		Sun's Meridian Passage 3m. 1·69s. before Mean Noon			6 26·9.
Wed	19	23 51 14 48 15 11	○ Full Moon Occultation of B.A.C. 1746 (6½) Reappearance of ditto			6 24·9
Thur	20					6 23·0
Fri	21	19 29 19 36	Occultation of 84 Geminorum (6½) Reappearance of ditto			6 21·1
Sat	22	9 4 10 35 12 26 13 38 12 35 13 26 12 40 13 48	Near approach of η Cancri (6) Near approach of 35 (6½) Occultation of ε Cancri (6) Reappearance of ditto Occultation of 42 Cancri (6½) Reappearance of ditto Occultation of B.A.C. 2925 (6½) Reappearance of ditto	Jupiter too near the Sun.		6 19·2
Sun	23	11 35 12 32	Occultation of 7 Leonis (6½) Reappearance of ditto			6 17·3
Mon	24	9 42 9 59 9 59	Occultation of 44 Leonis (6) Reappearance of ditto Near approach of B.A.C. 3562 (6½)			6 15·4
Tues	25	11 37	Occultation reappearance of 76 Leonis (6)			6 13·5
Wed	26	18 19	☾ Moon's Last Quarter			6 11·6
Thur	27					6 9·7
Fri	28					6 7·9
Sat	29		Conjunction of Venus and μ Capricorni 0° 7′ N.			6 6·0
Sun	30	18 12	Occultation reappearance of 4 Scorpii (6)			6 4·2
Mon	31					6 2·3
18 JA Tues	78 N. 1					6 0·5

THE PLANETS FOR DECEMBER.

AT TRANSIT OVER THE MERIDIAN OF GREENWICH.

Planets.	Date.	Rt. Ascension.	Declination.	Diameter.	Meridian Passage.
		h. m. s.	° ′		h. m.
Mercury ...	1st	17 12 50	S. 24 52½	4″·8	0 31·0
	9th	18 7 15	S. 25 41	5″·2	0 53·8
	17th	18 59 50	S. 24 56	5″·6	1 14·8
	25th	12 43 10	S. 22 43½	6″·8	1 16·5
Venus ...	1st	19 56 35	S. 23 30	22″·2	3 14·2
	9th	20 31 49	S. 21 21½	24″·2	3 17·9
	17th	21 4 13	S. 18 45	26″·6	3 18·8
	25th	21 33 14	S. 15 49	29″·4	3 16·3
Mars ...	1st	23 44 45	S. 2 24	13″·5	7 1·8
	9th	23 59 21	S. 0 30	12″·5	6 44·9
	17th	0 14 47	N. 1 26	11″·5	6 28·9
	25th	0 30 54	N. 3 24½	10″·7	6 13·5
Saturn ...	1st	23 3 37	S. 8 19½	15″·9	6 20·8
	9th	23 4 34	S. 8 12	15″·6	5 50·3
	17th	23 5 55	S. 8 2	15″·4	5 20·2
	25th	23 7 38	S. 7 50	15″·2	4 50·4
Neptune ...	3rd	2 14 15	N. 11 33	...	9 23·0
	19th	2 13 8	N. 11 28	...	8 19·0

Mercury sets 20 minutes after sunset at the beginning of the month, the interval increasing.

Venus may be observed on the 1st for about three hours and a quarter after sunset, the interval increasing.

Mars sets about an hour after midnight on the 1st, the interval decreasing to half an hour after midnight on the 31st.

Saturn sets a quarter of an hour before midnight at the beginning of the month, and then earlier each night. On the last day he sets just before 10 p.m. He is at quadrature with the sun on the 5th.

LUNAR OBJECTS SUITABLE FOR OBSERVATION IN DECEMBER, 1877.

BY W. R. BIRT, F.R.A.S., F.M.S.

The lunar area to the north of II. I. τ (see *Astronomical Register* Nov., p. 302) is II. I. ω, which contains the western portion of the northern part of the valley J. J. Cassini, registered by the symbol II. I. ω 1. The north part of the west wall being II. I. ω 2. Just exterior to this wall on the west rises the peak ι of Schröter, γ of B. and M. and Neison, its symbol is II. I. ω 4; Schröter represents it as a mountain spur from the wall II. I. ω 2 of J. J. Cassini, with a long shadow towards the west; B. and M. as a triangular table-land or plateau, the apex towards the south and steep sloping sides east and west. Neison says this plateau is crowned by three peaks, that on the west, II. I. ω 6 being 8,729 English feet high, those on the east 7,328 and 5,480 English feet, these two are included in the symbol II. I. ω 7 as that of the east side of the plateau. To the south of this plateau and apparently branching westward from the wall of J. J. Cassini is a mountain II. I. ω 5 rising to the height of 3,200 English feet, it is situated in N. lat. 71° 40′ and 17° 24′ E. long. Schröter appears to have overlooked it. II. I. ω 3 is a peak on the east border of Anaxagoras, attaining an altitude of 9,528 English feet.

The terminal basin of J. J. Cassini, H of Schröter and Anaxagoras γ B η Z of B. and M., as given by Schafarik in the October number of the *Astronomical Register*, p. 265, being near the north pole, occupies a large space, viz., from 22° to 50° E. long. The peaks B (not β as given by Scafarik) is in 22° E. long., 78° N. lat.; η is in 41 E. long., 82 N. lat., and Z in 50° E. long., 82° N. lat. The symbols are respectively of B II. N. ε 1, of η II. O. ι 1, of Z II. O. κ 1. From this latter point the east wall crosses the meridians of 45′ to 30° E., whence it wends south-west until it reaches area II. I. This area includes the eastern portion of the northern part of the valley J. J. Cassini II. I. with its wall II. I. 2, also a peak on this wall situated in N. lat. 70° 34′ E. long. 22° 40′.

Errata.—October list, par. II. I. ξ 3, last line but two, for *position* read *portion.*

November list, second par., last line, for *v* read the *Greek upsilon.* Next par., line 3, for *heights* read *height.*

Buckhurst Hill: November 16, 1877.

THE FIVE INNER SATELLITES OF SATURN.

Longitudes of the five inner satellites reckoned from upper conjunction, and expressed in decimal fractions of a revolution.

8h.Gr. Dec.	Mi.	En.	Te.	Di.	Rh.
1	·704	·841	·017	·927	·106
2	·765	·571	·547	·292	·327
3	·826	·300	·076	·657	·548
4	·886	·030	·606	·022	·770
5	·947	·759	·035	·388	·991
6	·008	·489	·665	·753	·212
7	·069	·219	·195	·118	·433
8	·130	·949	·724	·484	·655
9	·190	·678	·254	·849	·876
10	·251	·408	·783	·214	·097
11	·312	·138	·313	·579	·318
12	·373	·867	·842	·945	·539
13	·434	·597	·372	·310	·761
14	·495	·327	·901	·675	·982
15	·556	·056	·431	·040	·203
16	·617	·786	·961	·405	·424
17	·678	·515	·490	·771	·646
18	·738	·245	·020	·136	·867
19	·799	·975	·549	·501	·088
20	·860	·704	·079	·866	·309
21	·921	·434	·608	·231	·530
22	·982	·163	·138	·597	·752
23	·043	·893	·667	·962	·973
24	·103	·623	·197	·327	·194
25	·164	·352	·726	·692	·415
26	·225	·082	·256	·057	·636

To find the longitudes for other hours and the corresponding places of the satellites v. the tables p. 278.

Approximate times of some of the conjunctions of the satellites with the ends of the ring, v. p. 279.

1877.	Gr. M. Time. h.		
Dec. 1	5·0	Encel.	w.
	8·9	Dione.	np.
	9·1	Mimas.	w.
	9·9	Encel.	np.
2	7·7	Mimas.	w.
	8·9	Encel.	sp.
	9·4	Tethys.	sp.
3	6·3	Mimas.	w.

1877.	Gr. M. Time. h.		
Dec. 3	6·3	Encel.	e.
	7·4	Rhea.	sp.
	8·1	Tethys.	nf.
4	6·8	Tethys.	sp.
	10·3	Encel.	nf.
	10·5	Dione.	nf.
5	5·4	Tethys.	nf.
	7·7	Encel.	w.
	11·4	Dione.	sf.
6	5·1	Encel.	sf.
7	4·2	Dione.	nf.
	9·0	Encel.	e.
8	5·1	Dione.	sf.
	6·4	Encel.	np.
9	5·4	Encel.	sp.
10	7·7	Encel.	sf.
	8·0	Mimas.	e.
11	6·8	Dione.	sp.

1877.	Gr. M. Time. h.		
Dec. 12	7·7	Dione.	np.
	8·3	Rhea.	sp.
13	10·2	Tethys.	sf.
14	5·3	Rhea.	np.
	5·5	Encel.	e.
	8·9	Tethys.	np.
15	7·6	Tethys.	sf.
	9·3	Dione.	nf.
16	6·2	Tethys.	np.
	6·8	Encel.	w.
17	4·9	Tethys.	sf.
18	8·2	Encel.	e.
19	9·3	Tethys.	sp.
20	8·0	Tethys.	nf.
21	6·6	Tethys.	sp.
22	5·6	Dione.	sp.
23	6·3	Rhea.	np.
	6·5	Dione.	np.

To watch the shadow of Titan crossing the central meridian of the disc of Saturn, observers must be on the alert on Dec. 9, about 6½h., and on Christmas Day about 5½h. G. M. T.

Japetus is, during December, on the preceding side of Saturn, reaching on Dec. 16 its greatest western elongation. A. M.

ASTRONOMICAL REGISTER—Subscriptions received by the Editor.

To Sept., 1877.

Brothers, A.
Elliott, R.

To Oct., 1877.

Calver, G.

To Dec., 1877.

Campbell, Colonel.
Lewis, H. K.

To June, 1878.

Main, Rev. R.

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